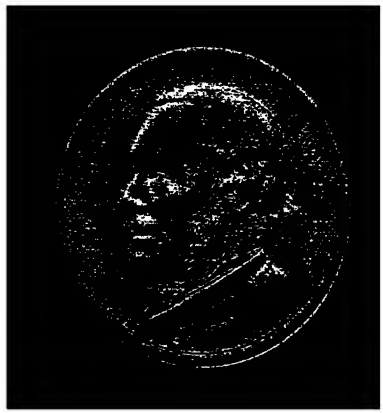
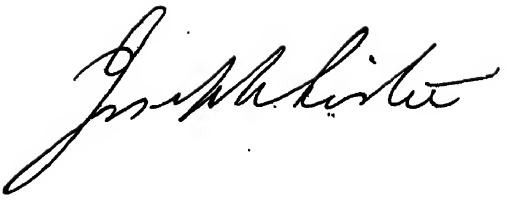


LISTER AND HIS ACHIEVEMENT



LISTER
AND HIS ACHIEVEMENT

BEING THE
FIRST LISTER MEMORIAL
LECTURE DELIVERED AT THE
ROYAL COLLEGE OF SURGEONS
OF ENGLAND ON MAY 14, 1925

By
SIR WM. WATSON CHEYNE, Bt.
K.C.M.G., C.B., F.R.C.S., F.R.S., Etc.

WITH APPENDIX

WITH PORTRAIT

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PREFACE

THE following is the Listerian Lecture in connection with the first award of the Listerian Medal and Prize by the Council of the Royal College of Surgeons of England. The wish of the lecturer was to give a short history of Lister's work, but that was impossible in the case of a short lecture. Hence the lecture was written out in full and compressed as much as possible. The lighter portions, illustrating Lister's character and the general plan of his work, were then used to form the Listerian Lecture, and the remaining more detailed account of the progress of his work and investigations is added in the form of an Appendix.

FETLAR, SHETLAND.
May 14, 1925.

LISTER

LISTER AND HIS ACHIEVEMENT

Mr. President, Ladies and Gentlemen,—Before I begin this lecture I should like to say how much we all regret the sudden death of Sir Rickman Godlee, which has cast a gloom over to-day's proceedings. Sir Rickman Godlee took a very active part in the Lister Memorial Funds, and was, I know, looking forward very much to the proceedings to-day : I also had been looking forward to meeting him. Sir Rickman Godlee and I were very closely associated for many years as Lister's private assistants, and a more delightful, able, and loyal colleague I cannot imagine. I am sure that everyone here sympathises most sincerely with Lady Godlee in her great bereavement.

I must, in the first place, thank the Council of this College for the great honour they have done me in awarding to me the first Lister Memorial Medal and Prize. I need hardly say that the Medal commemorating my great master and friend will be one of the most treasured of my possessions.

The subject of the lecture which must be given by the recipient of this honour is left entirely to his judgment, and will naturally refer in the main to work in which he has himself been principally engaged. On this occasion I feel convinced that, as the first recipient

of this honour, and as one of the little band of men [1] who knew Lister and his work most intimately, and who had the extraordinary good fortune of being closely associated with him throughout his great achievement, I ought to put on record matters connected with his work and not especially concerning myself. I hope that my decision will be acceptable to you.

As a matter of fact I have for some time had a great desire, before I depart this life, to place before the profession as fair a statement as I can of Lister's great achievement, his aims, the principles on which he started, how they became modified as time went on, and the final result. Having, however, practically given up active surgical work, I have felt more and more diffidence in laying before the profession a story which to some may appear to be past history and not of any actual living value at the present time. This award, however, provides the stimulus and, indeed, the immediate necessity for carrying out my desire. The story I have to tell, though going back for just sixty years, is one still full of vitality, of instruction, and of the gravest importance to surgery. It is far from being a closed book, the whole matter is still fluid, and perfection has not yet been obtained in all directions.

When I came to write the lecture on these matters in full I found that, even after cutting it down as far as possible by omitting extensive passages and by condensing others very much, its delivery would far

[1] In Glasgow, Lister's private assistant was Sir Hector Cameron, who, I am glad to say, is still well and active; in Edinburgh his assistant was Dr. John Bishop, who died shortly after Lister went to London—a most lovable man and deeply regretted by all who knew him. In London his assistants were Sir Rickman Godlee and myself.

exceed the time at my disposal and try your patience to breaking-point. At the same time I wished to make it complete, and I have, therefore, attempted to solve the problem by taking out of the original lecture the more personal matters which seem most suitable for a general discourse (and my own experience is that the less the dry detail in a lecture the more successful it is), and publishing in the same volume with this discourse the remainder of what I wrote originally, which can be perused and pondered over at leisure.

The story that I have to tell to-day is one of the most fascinating tales that I know, and far exceeds in interest the most exciting detective story ever written. And its influence on the life and health and usefulness of mankind surpasses all imagination; and it is a true story. It is the story of a fight against a group of diseases—the infective or septic diseases of wounds—which have attacked wounds probably since the world began. As a matter of fact any wound of the skin, whether as the result of an accident or of an operation deliberately performed by the surgeon, involved up till Lister's time a very great risk to the patient of serious illnesses called 'septic diseases,' which, indeed, very often proved fatal. And although numerous attempts had been made by surgeons of all countries and through many ages to discover the cause and to find a remedy for these diseases, no real progress was made till Lister took up the work. Up to that time a surgical ward in a hospital was a place in which most of the patients were visibly ill, with flushed faces, parched lips, delirium, severe pain, etc., and many of

them were evidently on the verge of death; the wards were pervaded with a peculiar mawkish odour which was very trying to newcomers. I suppose very few here to-day remember those days. What a contrast to the surgical ward of to-day, with its fresh air, happy and contented patients looking as if nothing had happened to them, and very slight mortality. This we owe to Lister, and much more, for not only has his work led to the practical disappearance of septic diseases, but as a result it has enabled the surgeon to perform many operations which prolong life, restore movements, rectify deformities, and add to the usefulness and comfort of mankind.

Lister's first introduction to septic diseases was when he became a dresser in the surgical wards at University College Hospital, London. I think that his very first case had been attacked by phagedænic gangrene, one of the most terrible of these septic diseases. That and other similar cases impressed him so greatly that he never forgot them, and always felt that no matter was more urgent or might be more fruitful than the attempt to find out the cause, the prevention, and the best methods of treatment of these septic diseases.

We may here pass over his earlier years and come to the time when he was Professor of Surgery in Glasgow, and was appointed one of the surgeons at the Glasgow Royal Infirmary. This was late in 1861, and by that time Lister had done excellent experimental work on the early stages of inflammation, coagulation of blood, etc.—work which was to be of the greatest

value to him in future and for which, I may add, he had been elected at the early age of thirty-three a Fellow of the Royal Society.

At this time he had come definitely to the conclusion that sepsis was closely bound up with putrefaction of the blood and discharges in the wound, and that, if he could only prevent this putrefaction, septic diseases would in all probability disappear; the cause of putrefaction was, however, still unknown. Most surgeons attributed it to the action of the gases of the air, especially of the oxygen, on the organic fluids in the wound, while others presupposed the existence and action of indefinite bodies which they called 'miasms,' and which were supposed to develop in unhealthy regions, overcrowded wards, etc., and pass into the air. On the oxygen theory many attempts had been made in former years to prevent the access of air to the wounds, but without any improvement as regards sepsis, and on the assumption that the gases in the air or 'miasms' in the air, which were also supposed to be gaseous, were the cause of the trouble, Lister could not devise any preventive means. The only thing that suggested itself was to try to diminish the effect of the putrid products by keeping the wounds as clean as possible, and so he carried out an extensive experiment in his wards with ordinary cleanliness, but without success, and one can quite imagine his depression when he found that the best methods that he could think of did not help him at all and that pyæmia and all the other septic troubles dogged his footsteps and rendered his best work nugatory.

But it so happened, though Lister did not then

know it, that the cause of putrefaction was being revealed about the time that he was appointed Professor of Surgery in Glasgow. For over a century a very interesting, and sometimes a very heated, controversy had gone on as to the origin of life, and in the first instance the controversy was carried on chiefly by priests. Indeed, I presume that it began on some such theological problem as this: Was there only one creation of life, as suggested in the Bible, or is it possible that the conglomeration of certain atoms in certain ways may, shall we say, attract the vital spark (just as an electric current may be set up by the contact of various metals), and that in this way a new living organism might be produced? The advocates of the latter view, which came to be spoken of as 'spontaneous generation,' sought to support their opinion by elaborate experiments which were quickly disposed of by their opponents, but only to lead to a fresh crop of experiments. One would never have thought that such an abstruse matter would have any practical influence on commerce, and still less on the lives and health of living beings. As regards the former, however, the subject of fermentations became involved, and it then became a matter of importance to chemists. About 1860, Pasteur not only closed the proof against the spontaneous generation of yeast cells, but also, I believe quite unwittingly, opened up a new and, as it ultimately turned out, an extremely vital line of research by pointing out that other fermentations apart from the vinous, such as the butyric and the putrefactive, were also dependent on living organisms, which gained access to the putrescible fluids from the dust in the air or on surrounding objects,

and especially from other putrefying media. Pasteur himself does not seem to have had at the time any idea of the importance of this part of his investigations, and, as far as I can ascertain, only three men at all thoroughly realised it, or at any rate made any attempt to utilise the knowledge within the next few years. These men were Jules Lemaire, Spencer Wells, and Joseph Lister. It will be of interest for the moment to sketch out what each of these men did with the knowledge which they acquired from Pasteur's work ; in doing so I do not want in any way to compare these three men, but, as a matter of fact, just as no two persons have exactly the same finger-prints, so I believe that no two persons have exactly the same arrangement of brain, and, as thought and action are the results of the working of the brain, no two persons will take exactly the same steps in the solution of a problem. Thus it is very interesting to compare the result of the action of different types of brain.

Jules Lemaire was, I fancy, of the type of the delightful, old, learned family doctor. He was evidently fond of surgery, but he had no surgical hospital appointment and so had not the opportunity of testing any views he may have formed ; he was also much interested in scientific matters. He saw at once that Pasteur's statement as to the cause of putrefaction was of great importance. He wrote a good deal on the bad effects which might arise from bacteria, and he washed his wounds with various antiseptics, notably with carbolic acid, with the view of interfering with the growth of bacteria in the discharge. He never, however, suggested, as Lister did, that these anti-

septics should be used to kill the germs before they got a footing in the body or in the wound, and he spoilt his case by advocating the use of carbolic acid for all sorts of diseases. Evidently Lemaire's advocacy of carbolic acid had attracted attention, for Declat thought it worth his while to claim priority in the use of the drug.

Something like three or four years elapsed, and then I find that Spencer Wells, one of the pioneers in ovariotomy and subsequently President of this College, in an address at the meeting of the British Medical Association in 1864,[1] gave a most excellent description of Pasteur's work and its bearing on surgery. I do not think that a better résumé could have been made. Spencer Wells was a great practical surgeon and a bold operator, but, curiously enough, he did not follow up the logical conclusions that he drew from Pasteur's works. Indeed, his only remark as to the practical application of the work to which he called attention was that 'surgeons should always secure the most scrupulous cleanliness and purity of everything surrounding them,' and as regards any other treatment he advocates the internal administration of sulphites in the form of sulphite or hyposulphite of soda.

There is no doubt that the recognition of the value of scrupulous cleanliness in the sense of preventing the putrefactive germs from getting a footing was a very important advance, but a good deal more was needed for perfection.

The third man, so far as I can find, who took up Pasteur's work was Joseph Lister. I cannot make

[1] *British Medical Journal*, vol. ii, 1864, p. 384.

out exactly when this knowledge first came to him, but I fancy it was in 1864. I understand that he was walking home with his colleague, Professor Thomas Anderson, the Professor of Chemistry in Glasgow, one afternoon towards the end of 1864, discussing putrefaction and its causes, when the latter suggested that he should read certain papers on fermentation and spontaneous generation which had been recently published by a rising young French chemist, Louis Pasteur. Lister, who naturally was not conversant with chemical literature, had not heard of Pasteur's work, nor had he followed the controversy on spontaneous generation, but he at once got these papers,[1] and from the perusal of them he found that Pasteur's experiments showed that it was not the gases of the air that the surgeon had to fear, but minute living particles floating in the air and settling on surrounding objects in the form of dust. And he learned further that so long as these particles were excluded from the putrescible material after it had been boiled there was no sign of living organisms or putrefaction in the material. As soon, however, as the protection was removed and the dust was allowed free admission, minute bodies belonging to the lowest class of vegetable life could be seen swarming in the fluid, and putrefaction very quickly set in.

The conclusion was that some of the particles which were excluded by filtration or destroyed by heat were actually living germs which, as they developed in the putrescible material, set up the putrefactive fermentation just as the vinous fermentation was only set up on the admission of living yeast-cells.

[1] See *Œuvres de Pasteur*, by Vallery-Radot, vol. xi, 1922.

One can well imagine Lister's excitement and delight when he realised from Pasteur's work that the enemy with which he had to deal was not a gas present everywhere and practically impossible to get rid of or avoid, but living solid particles floating in the air or present in the dust, capable of being killed or avoided—still a difficult problem no doubt, but not an insoluble one. These 'germs,' as they were called for a long time, were new to him and to the surgical profession generally. Long rods, known as vibrios, were comparatively easily seen swimming about in putrefying fluids, and also small rods and round bodies; but they had not been specially studied, and to many it seemed the height of absurdity to suppose that such minute bodies could be of any practical importance to the surgeon.

I do not think that at this time Lister, nor, indeed, anyone else, had any very definite idea as to the *modus operandi* of these germs. It was enough for him for the time being that they were the cause of the putrefaction of the discharges from the wounds, and that the putrid fluids were somehow or other, if not the cause, at least very closely associated with the causes of local and general septic diseases. Lister's application of Pasteur's statements was :

(1) That these germs must be prevented from getting into wounds during and after operations, and that they must be killed if possible outside the wound.

(2) That if they have succeeded in getting into a wound before the case is seen by the surgeon, as in compound fracture, they must be killed if possible before they have had time to spread and secure a

footing, and further germs must then be prevented from entering the wound afterwards.

Lister's method of dealing with the information gained from Pasteur's work was thus more definite and more drastic than the suggestions of the other two surgeons referred to, and reflects the action of the philosophical and experimental type of brain. He never swerved from his first decision that the aim of the surgeon must be the prevention of the entrance of living germs into the wounds, or their destruction, if possible, before they can establish a footing. But it will be seen that, as time went on, he found that it would suffice under certain circumstances, especially in the interval between the dressings, to inhibit their growth without necessarily killing them. That his suggestion was the most fruitful of the three is evident from the further course of events.

On further consideration of the various ways of commencing this work, it seemed to Lister that it would be best to select cases where the germs had had an opportunity of getting into the wound before the patient came under treatment, but where only a short time had elapsed since the injury—so short that the germs had probably not yet had time to gain a firm footing. Such cases were compound fractures, where the patient comes under the surgeon's care shortly after the accident, with the wound already infected, cases which were notoriously extremely dangerous, large numbers of patients with compound fracture dying of septic disease. In a vast experiment affecting the whole domain of surgery such as the facts suggested to Lister, the responsibility of the surgeon was at the

beginning, of course, very great, lest instead of doing good he should make matters worse; but in compound fractures the prospects of the patients could hardly be made worse, and there was a very considerable chance that they might be much improved. And in addition, the experience that he would gain in treating compound fractures might help him in dealing with operation wounds, which was a larger and much more complicated problem. Besides, except in very urgent cases, operations through unbroken skin could be delayed till he saw what happened in compound fractures.

As regards the procedure, all the laboratory experiments on fermentation and spontaneous generation had so far depended on the employment of heat to kill the germs, or on filtration of the air to remove them; but neither of these plans seemed suitable for the present purpose, and therefore Lister turned his attention to chemical substances in the hope of finding one which would kill germs without seriously injuring the tissues exposed in the wound. Curiously enough, during 1864 he had been attracted by a notice in the newspapers about the remarkable results on the sewage of the town of Carlisle by the use of carbolic acid—a small quantity not only preventing the smell, but also apparently destroying the entozoa which usually infected cattle fed on the pastures on which the sewage was placed. Professor Anderson obtained for him a supply of this acid, which was sold under the name of German creosote. This substance was very impure and did not dissolve at all well in water; in fact, Lister stated at first that carbolic acid was not soluble in

water, but later on, when he got a purer specimen, he found that it dissolved readily enough up to as much as one part of the acid in twenty parts of water, and in linseed oil in almost any proportion ; the oily solution, however, did not seem nearly so active as the watery one.

It so happened that Lister had to wait a considerable time before a suitable case for his purpose was admitted to the wards. In fact, he had to wait something like ten months before his first real attempt at treatment was made. While we are waiting for the case of compound fracture to come into the ward, I may occupy the time by giving you an account of the man himself and of the chief influences on his life.

I like to think of Lister, with his courtly manners and indomitable courage, as one of the knights of olden times sallying out single-handed to find and destroy a formidable enemy, armed with a great but imperfect generalisation, or rather, working hypothesis, and a bottle of crude carbolic acid so impure that he tells us in his first paper that carbolic acid was insoluble in water. He had never seen his enemy but he saw his ravages on every hand; he did not know how he acted in producing diseases; he did not know where he lived or how he lived. He knew nothing about the various protective substances which are spoken about at the present time, and perhaps it is well that he did not, for he might have been so bewildered by the various ways in which it is supposed that the enemy on the one hand and the animal forces on the other act, that he might not have known where to strike or might have feared to strike at all, and so have failed completely. All that he knew was that

the enemy was probably a unicellular body belonging to the vegetable kingdom and of extreme minuteness, not exceeding more than $\frac{1}{20000}$ of an inch in diameter. All that he cared for or needed to know was that he had a terrible enemy who, however, probably had his vulnerable point, and that the gain was so great, if the battle were won, that it was well worth while to devote his whole mind and his whole energies and his whole life to the solution of the problem.

And it was a great fight, not merely in studying the tactics of the enemy and in devising better and better means of preventing his access to the wounds or of counteracting the effects of his growth and multiplication in them if he had already entered, but also in facing great and unaccountable prejudice and ridicule. This fight was one which few could have carried out to its end, but Lister's profound conviction of the truth and value of what he was battling for, viz. the abolition of sepsis on the lines laid down in his writings, and of the enormous benefit to mankind if the problem were successfully solved, enabled him to put prejudice and ridicule aside and leave the verdict to the future. But he was much helped by a variety of circumstances.

He was mentally well fitted for the task which he had undertaken. He had one of the attributes of genius well developed, viz. that he did not overlook apparent minutiæ. Nor was he led away by the absurd copybook proverbs—'There are exceptions to every rule,' and 'The exception proves the rule.' If he found an exception to a supposed rule he wanted to know why; he did not pass it over as if it were

merely an exception and therefore of no consequence. Pasteur had this faculty perhaps still more developed, and Koch also possessed it, and with all three men some part of their success depended on the fact that they were not content to regard deviations from the ordinary course as trifling but wanted to know why these deviations occurred, and in investigating them came across unexpected and helpful information.[1]

Further, Lister had a very logical brain and a very open mind; he was ready to modify his views and plans at once if proper evidence demanded it. He was full of sympathy for suffering, and would devote himself to the relief of a patient without consideration of time or trouble. I think that probably his greatest characteristic was his conscientiousness. Before he took action on any point he turned the matter over and over in his mind so as to be as sure as possible that his action was right. His conscientiousness had a very curious result as regards his public appearances; indeed, he became rather notorious for being late when he had an important address to give, the reason being that on thinking over what he wished to say something was apt to occur to him at the last moment which he thought he ought to verify before he spoke. I remember when he gave the Bradshaw Lecture here that the Council assembled, but there was no sign of the lecturer, and at last they filed into this room and took their places so as to calm the audience, but he was still

[1] To change my metaphor for the moment, the work of these three men was needed to construct the building properly. Lister conceived and carried out its erection, Pasteur laid its foundation 'well and truly,' and Koch assisted very materially by his work on disinfection and also clarified the structural details by his work on infective diseases of wounds and on the cultivation of bacteria on solid media.

missing. I was sitting on the back benches and, spying me, messengers were repeatedly sent up to me to ask what had happened to Lister and if he was coming. I could only say that I had left him at 3.30 and that he was then quite ready; but sure enough, at the very last moment, something struck him which he felt that he ought to look into before he came down.

I have been told that on one very important occasion, when he was giving an address on coagulation of the blood, an idea occurred to him in the carriage on his way to the lecture hall, and that he turned into a butcher's and had an animal killed and the observation made before he went on to his lecture. And in his private practice I have known him to be an hour or two late for a consultation, and sometimes when he ought to have been at his house for his appointments he would look in at his nursing home in passing, and if anyone said that they did not feel quite comfortable he would take off his coat and re-adjust dressings or splints, quite regardless of the fact that his waiting-room was full of irate doctors and patients vowing that they would never consult him again.

But apart from his own endowments, which he was constantly improving by hard work, he was one of a family of exceptionally able people. Especially did he owe much to the influence of his father, who was greatly interested in optics, which were his chief hobby and recreation; indeed, it was he who developed the achromatic lens, and for his work in this direction he was elected a Fellow of the Royal Society. In Godlee's charming Life of Lister he reproduces much correspondence which took place between father and

son, and which shows their great intimacy, the care with which he kept his father acquainted with all the work that he was doing, and the wise counsel which his father gave him.

And then the whole-hearted way in which his wife entered into his work and acted as his intimate adviser and secretary was quite invaluable for Lister. Lady Lister was the eldest daughter of Mr. Syme, and they were married in 1856. She was one of the most beautiful characters one can meet with, and she was an ideal wife for Lister. A similar Lister Memorial Fund to this has recently been established in Canada, and the first Lister Memorial oration was delivered last year by Dr. John Stewart, who was one of those who came with Lister to London in 1877 to help in organising the ward work. In his excellent address I find that there are a few remarks about Lady Lister which I cannot refrain from reproducing here, and I am sure with Dr. Stewart's full approval. I assume that the words which I am going to quote were spoken at the wedding feast in what, I fancy, was a speech of congratulation to the newly married couple by Dr. John Brown, author of 'Rab and his Friends,' among other essays, a well-known, learned, and much-beloved physician in Edinburgh. Dr. John Brown in his remarks referred to an illness which Miss Syme had suffered from when a little child, and in which she had lain unconscious for several days. He spoke as follows: 'Lister is one who, I believe, will go to the very top of his profession, and as for Agnes, she was once in heaven for three or four days when she was a very little child, and she has borne the mark of it

ever since.' Nothing more beautifully and more truly expressive of Lady Lister's character could be said than that. She was a true helpmate, spending hours with Lister helping him in his scientific work and taking notes for him; indeed, the notes of Lister's experiments which he has left behind him are for the most part in Lady Lister's handwriting.

Apart from the above factors, Lister was much comforted by the approval and interest of men whose opinion he especially valued. In his early days two of his teachers at University College, London, had a great influence on him, viz. Professor Sharpey, the physiologist, and Professor Wharton Jones, the Professor of Ophthalmic Surgery, but the man who had the greatest influence on him was Professor Syme. Lister had the very highest opinion of Syme as a man and as a surgeon, and the fact that Syme approved of and latterly became enthusiastic about his work was an immense gratification and encouragement to him, and enabled him to ignore the captious criticisms of his opponents.

And then lastly, and by no means least, he was much encouraged by the enthusiasm of his pupils. I do not know of anyone who excited such enthusiasm and had such influence over his students as Lister had, especially in Glasgow and Edinburgh. The large and enthusiastic classes which he had there were a great incentive to him.

I have now mentioned some of the chief influences which helped Lister to undertake and carry to success

the battle against sepsis, and, having waited long enough for the case of compound fracture to turn up, I may say something by way of example about the methods employed and the results obtained. I think that the way in which he commenced his campaign is most characteristic of his conscientiousness and care in all his work. Most men, I fancy, having arrived at the conclusions to which Lister had come, would have been on tenter-hooks to try the methods and would very likely have applied them to any case which seemed at all suitable, very possibly with disaster and disillusionment. But Lister had the patience to wait till a suitable case for the experiment turned up, and I believe that during that time he was further planning out his campaign and also repeating some of Pasteur's experiments and devising some of his own.

His first case of compound fracture did not come under his care till March 1865, but no details are given, and all that Lister says about it is that 'it proved unsuccessful in consequence, as I now believe, of improper management.'

I need not go into the eleven cases which he published in his first paper. The results presented a great contrast to those that he had been obtaining previously.[1] I may, however, mention one or two

[1] As a matter of fact, Lister overrated his first success because at that time he looked on the presence or absence of *putrefaction* as the criterion of failure or success—and most of the cases were free from putrefaction. It was not till later that he adopted *suppuration* as a truer criterion as regards sepsis, for septic diseases might develop in connection with a wound that was suppurating but the discharge from which was not putrid. The absence of suppuration is a much more useful test of asepsis than the absence of putrefaction. Even the absence of suppuration may theoretically not be an absolutely true guide, for cases of erysipelas occur without suppura-

observations that he made on these early cases, which were new to him and of the greatest interest and importance.

In the first place, these cases gave him a high opinion of the value of carbolic acid for his work; indeed, it is very interesting that this substance, chosen more or less at haphazard, has turned out to be the most useful all-round antiseptic. The cases specially referred to are the second and fifth cases in his first publication. Put shortly, only two of the eleven cases developed phagedænic gangrene, a smaller proportion than usual; but the point that he makes is that in the one (No. 2) the disease developed after he had left off the use of the carbolic acid, which he thought had done its work, and in the other (No. 5) it broke out at a small wound which had been overlooked and had not been treated with carbolic acid at all: it had spread from this little wound to the lower part of the bigger wound before it was noticed. In both cases recovery took place, but after a long struggle.

Let us now take only one example of the new facts

tion, and it is conceivable that tetanus might do so also. But taken as a whole the presence or absence of suppuration is quite satisfactory as a test of failure or success as regards asepsis.

On this criterion I find that five of the eleven cases in Lister's first paper were typically aseptic cases, five were septic and in one the sepsis seems to have been very slight indeed. Surely that is a very remarkable result for a first attempt in cases where it must always be a matter of doubt whether one has satisfactorily disinfected every recess of the wound or not. I have no hesitation in saying that up to that time Lister had never seen five cases out of eleven running an aseptic course, indeed I doubt if he had ever seen one.

As to mortality, of these eleven cases only one died (from perforation of the popliteal artery by a spicule of bone on which it rested, 3½ months after the injury), a mortality of nine per cent., a very low mortality as cases went at that time.

which he met with in his cases, viz., the behaviour of blood clot in aseptic wounds; this was quite new to Lister. He applied carbolic acid to all the torn tissues and broken bones with a view of killing the bacteria which had got in, and he thus left a mixture of blood clot and torn tissues in the wound impregnated with carbolic acid. In these cases he noticed that the blood clot did not contract and break down as he was accustomed to see it do, but remained firm and adherent to the sides of the wound.

That was the first fact; now for the second. On taking out a piece of the clot after about a fortnight and examining it under the microscope, he found that it contained numerous connective-tissue cells, some in bundles; later on blood-vessels were beginning to develop.

A third fact was that pieces of dead tissue mixed up with the clot also became infiltrated with connective-tissue cells in the same way as the clot, so that not only did organisation occur in the dead clot but also in the more substantial pieces of dead tissue, as the result of the penetration of these connective-tissue cells into them and the subsequent disappearance of the dead tissue. This last fact was what chiefly led him to take up his work on catgut and ligatures generally.

And a fourth fact was that in some cases, after two or three weeks one would find that the edge of a thin layer of clot on the surface could be lifted up, and on doing so a layer of epithelium was disclosed spreading over the clot just below the surface and completing the process of cicatrisation, without any granulation and without any suppuration.

These points constitute the process of healing by blood clot, which we have taken advantage of ever since in cases where we could not conveniently bring the edges of the wound together. Curiously enough, this point seems to be lost sight of almost completely at the present time. The other day I picked up copies of four recently published books on general surgery, and in only one of them was there any mention of organisation of blood clot. This is a pity, because it is a most useful thing ; but for the reasons which I give towards the end of the Appendix, I doubt if exposed blood clot could be kept aseptic for a sufficient length of time to permit organisation where the technique of the surgeon excludes the use of chemical antiseptics.

Let us take one more example from the first case in which his method was applied to an operation wound. As the case is referred to in the Appendix I need not go into details here, but I may say that the case was one of psoas abscess, and having opened it and having squeezed out the pus from the abscess, he mixed it up with the carbolic acid to form a paste with which he dressed the wound. Just imagine the horror of the laboratory bacteriologist at such a suggestion at the present day ! Nevertheless, when Lister came to dress the wound next day, prepared to make a paste in the same way as he did the day before, he was astonished to find that there was no more pus, and that all that he could squeeze out of the wound was one or two drops of clear serum ! The history of the case is that it remained aseptic and ultimately made a complete recovery.

I may now pass on to the beginning of the session 1872–73, when I first became acquainted personally with these matters in a curious way that may interest you. I began the actual study of medicine in May 1871, and in October 1872, when I was in my second year, I took out the classes of surgery, physiology and anatomy. There was an interval between the classes from twelve to one o'clock, and I did not know what to do with myself during this interval. My lodgings were a considerable distance away, and there was no object in walking there and back; what I wanted was shelter from the biting east winds and the wet. At that time there was no Students' Club at Edinburgh, as there is now, and the only thing I could think of was to go and sit in the general library. But it was small and crowded with students of all faculties, and there was no chance of a shy youth like myself getting a seat. So it occurred to me that I might as well take out my hospital ticket in October 1872 as in the following May, which would be the usual time; in going round the hospital I might learn something, and anyway I would get shelter from the wintry blasts between noon and one o'clock.

I shall never forget the first day that I went to the hospital. At twelve o'clock I crossed the road to the infirmary and found crowds of students hurrying along the lobbies, a good many, indeed, running. I joined the company, full of curiosity, and found that we were going to Lister's clinical lecture. We ultimately reached the large operating theatre and found it filled with men crowding the gallery and standing at the back and in the gangways: there must have

been over 200 there, and they were senior men, for the clinical surgery class was not usually attended till the fourth year. I should think that I was the only one of my year there. Presently a man walked into the arena, sat down on a chair placed on the floor of the theatre, and, crossing his legs and putting his right hand on his thigh, he began his lecture. This man was Lister. He had not spoken many minutes before I became fascinated with the subject and could not help seeing the wonderful era that was dawning for surgery. I had my notebooks for the other classes in my hand and I started to take down what he said, and, as he spoke very slowly and clearly and I scribbled very quickly, I was able to take down practically the whole lecture, though, not having learnt any surgery, I did not understand it at all clearly at first. But what a contrast with the lectures on systematic surgery! Very dreary performances full of curious theories about the reactions of the body and inflammation, quite unintelligible to me, evidently something which had to be memorised but nothing to think about. And then at twelve o'clock, simply seeking shelter from the 'cauld blasts o' Auld Reekie,' and not knowing that I had met my fate, I became entranced by the wonderful vision laid before us by Lister.

Is it any wonder that I left the theatre an enthusiast for the profession that I had chosen, and that on lecture days I was one of those who ran? An enthusiast I still remain. On my way home that first afternoon I bought a superior notebook into which, as soon as I got home, I copied out my notes of Lister's lecture and all that I could remember, looking up the words I could not understand in my text-book on

surgery. This I continued to do after every lecture, and this notebook, containing Lister's lectures for the winter session 1872–73, and part of the following summer session, I have presented to this College, and it will be found with the Lister relics if any one cares to look at them.[1]

At that time, and till Lister left Edinburgh, the rivalry among the students—between Lister's followers and those who continued the old system—was very marked. The Listerians, who were considerably the more numerous, looked on the non-Listerians as lost souls, Tories of the most die-hard description, and not likely, when they came to practise their profession, to be able to give their patients the best chance possible. The non-Listerians, on the other hand, looked on the others as crazy believers in vain things like germs, rash to a degree, blinded with their enthusiasm, placing their patients in the greatest danger by the outrageous treatment that they proposed, and, as they said that their wounds did not suppurate while those of the other side did, liars of the first water. This rivalry increased for a considerable time, and when I became Lister's house surgeon in Edinburgh in 1876–77 it was probably at its height. It was, I believe, of the

[1] In his lectures, Lister tried to show and discuss examples of as many different surgical diseases (even the simplest) as came to his clinic, so that in the course of the session the students had seen a considerable variety, especially of the ordinary cases which turn up in private practice. In my notebook we find among other things spinal caries, varicocele (with operation), epulis, spina bifida treated by drainage, suprapubic lithotomy (probably the first case performed for many years), excision of elbow, simple fracture, burns, ununited fractures with operation, stricture of œsophagus, stricture of the urethra, malignant tumours of jaw, nævus, hypospadias, cancrum oris, fistula in ano, strangulated hernia, etc.

greatest value to the cause of asepsis. It made us all extremely careful, because we knew that everything we did was closely watched and that no excuses would be accepted if a case went wrong. What we were most concerned about was that it would be the principle that would be blamed and not the individuals. By this time the antiseptic treatment had reached a stage where we felt that suppuration should not occur in an operation wound made through unbroken skin; if it did occur—and, as we were not perfect, one or two cases would suppurate during the six months' session—we all felt it very much: we went about as if everything were lost, ashamed to raise our heads, and subjected to merciless chaff. Our only topic of conversation among ourselves on such occasions was how the infection had got in, what was the error in technique which had been committed and whose fault it was; most excellent training and never forgotten; nothing could make us more careful than to be made to feel that suppuration in an operation wound was due to some error in our technique and was a serious reflection on the surgeon or the dresser. And I don't hesitate to say that I still agree with this axiom, and that in my opinion it holds true at the present time just as much as it did in the old days.[1]

[1] Not long ago I came across the following very suggestive passage in a paper by Dr. Thomas Keith, one of the great pioneer ovariotomists, on 'Ovariotomy before and after Antiseptics' (*Brit. Medical Journal*, October 1878):—'The regulations made in some hospitals that every visitor must sign his name in a book declaring that he has not for a week visited any case of infectious disease or attended a post-mortem examination, before being admitted into the operating room, has always seemed to me to be meant for a sort of *plaisanterie*. For my own part, when a case goes wrong after an operation, I have seldom to look far beyond myself for the cause of

By this time (1872 onwards) Lister's work had attracted more and more attention, and quite a number of surgeons began to visit Lister's wards. Curiously enough, very few British surgeons were to be seen, but other countries were well represented, especially Germany. Lister made a special point of showing all his methods and results to the visitors, and as there were constantly fresh arrivals he had to repeat his story frequently ; thus his hospital visits were somewhat prolonged. In the early days to which I specially allude the steam-spray producer had not yet been introduced, and it was very hard work to keep a hand spray going for any considerable time. The dressing of a patient followed a regular routine: the dresser on duty got on his knees at the side of the bed, and as soon as the bandages had been cut he started his hand spray. Lister then lifted off the outer dressing, which was solemnly handed round to each distinguished foreigner to smell. Having satisfied themselves that there was no putrefaction, the deeper piece of gauze which was generally placed over the region of the wound was passed round to show that there was no pus, and then in cases where it had not been possible to bring the edges of the wound completely together, and where blood clot could be seen in places between the edges of the incision, came the *pièce de résistance*. In such cases Lister usually covered the wound with a piece of material which was impermeable to carbolic acid and which he called 'protective,' and when this was exposed he would take a pair of forceps and peel off the protective,

failure—something done, something not done. This is a lesson hard to learn ; we blame persons, things, accidents, and circumstances rather than ourselves.'

exposing the wound with the adherent organising blood clot lying in the spaces where the edges had not come together, solid and firm, and with no sign of inflammation or suppuration in the wound. As a rule this was followed by a sort of gasp of surprise by the distinguished foreigners, and then a violent conversation would break out among them, accompanied by equally violent gesticulations, so that one became alarmed lest the peace of the nations was going to be endangered. The poor dresser, who was almost and, indeed, sometimes actually faint from the pumping of the spray, was for the time being completely forgotten. But, however exhausted he was and however much his wrist and arm ached, not one of his dressers would give in and let Lister down.

I have described in the Appendix the stage which the methods of treatment had reached by this time, and for some years Lister was probably most interested in revising the operative treatment of every disease or injury that presented itself from the point of view that he need not now fear sepsis in the operation wound. I have also referred in the Appendix to the first case of badly united fracture at the ankle joint which he rectified by open operation, opening the ankle joint freely. At first he did more bone work than anything else, ununited fractures, badly united fractures, deformities of bone (he was the first to operate for knock-knee, very much on the lines of the operation devised by Sir William Macewen some time later), wiring or pegging recent fractures, *e.g.* fractures of the patella or olecranon, operations for varicose veins, open radical cure of hernia, extensive operations for cancer of the breast, and so on.

He was the first to resuscitate suprapubic lithotomy, though this is generally attributed to others. In fact, his remarks on this matter will be found in my note-book, where you will see his reasons for this suggestion, but he was not very happy in their practical application.

From this time (1873) onwards Lister's activities followed two great directions which are both traced out in the Appendix. In the first place he continued his efforts to find further antiseptics which might be less irritating and more useful than those which he had hitherto employed, and he spent a great deal of time in testing various substances which were brought to his notice; and the other line of activity which he followed was to reconsider the treatment of all sorts of cases which came before him from the point of view of whether something better might not be done for them by operation than was being done at that time, acting on the knowledge that sepsis might be reckoned to have now been entirely abolished from wounds made by the surgeon through unbroken skin. It was probably in that direction more than in any other that Lister's work became so intensely interesting to the students. They would come to his wards and see treatment employed which had apparently never been heard of or thought of by the other surgeons who did not employ antiseptic measures. It would be hopeless to attempt to give any sort of list of the new operations which Lister did, and he himself absolutely declined to publish them.

When I became more intimately associated with Lister, I made repeated requests to be allowed to publish notes of his clinic, especially of the new

operations and new lines of treatment which he was employing; but although he had done that sort of thing for Mr. Syme, he pointed out, quite correctly, that the matter was very different, and that if such cases as opening joints, complicated operations on bones, and many others that might be mentioned, were published and were shown to have much benefited the patients, it might quite well happen that some surgeon who was not a believer in antiseptic treatment, or who was not sufficiently acquainted with the technique, might proceed to perform an operation on similar lines, and, being without the protection of the Listerian technique, the patient might lose his life or limb as a consequence. That is the explanation why so little is known of Lister as a surgeon. But, as I said before, I think that it was his fresh outlook on treatment and the interest of speculating what fresh procedure Lister would suggest in a particular case that fascinated the students most. The only sphere of operative work in which he did very little was abdominal surgery. Indeed, with the exception of ovariotomy, which was, curiously enough, supposed to be a very difficult operation and one sacred to a few specially gifted men; comparatively little work had been done in abdominal surgery as we now know it before Lister retired from practice, and he openly stated that he did not feel justified in embarking on surgery in a region with which he was not thoroughly familiar.

I became Lister's house surgeon in Edinburgh in October 1876, having spent the greater part of a year in Vienna and Strasburg, and I may say that in my visit abroad I found the greatest enthusiasm over

Lister and his work. Indeed, the fact that I had worked in his wards and was going to be his house surgeon on my return to Scotland was a sort of *Open Sesame*, and one of the professors actually asked me to come and show him exactly how Lister dealt with his cases.

It was in the spring of 1877 that Lister received and ultimately accepted an invitation to go to London as Professor of Clinical Surgery at King's College conjointly with Mr. John Wood. The first that I heard of it was one Sunday morning, I think a day or two after he received the invitation, when I was sleeping quietly in the house surgeon's bedroom. I woke up to find someone shaking me, and to my astonishment on opening my eyes I found that it was Lister. He told me about his invitation to London. He had not yet at all made up his mind about it, but if he went he would need to take a small staff with him familiar with his methods, and he had come down that morning to know whether, if he decided to go, I would go with him and again act as his house surgeon for six months at King's College Hospital. Go with Lister to London! I could not believe my ears! Of course I would go with him to London or anywhere else. When he ultimately made up his mind to go to London he took with him in addition Dr. John Stewart (now one of the leading surgeons in Canada and held in very great esteem there), Dr. W. H. Dobie of Chester (in large practice there), and Dr. Altham (who died soon afterwards) as clerks.

Lister hesitated very much about leaving Edinburgh and even now it is very difficult to say whether

he was right. There he had everything that could be desired, delightful society of the most congenial kind, large clinical department, numerous students all highly enthusiastic about his work; everything perfect so far as he personally was concerned. But antiseptic surgery although being widely adopted abroad was making slow progress indeed in this country. That grieved him very much and the enticing thing about the transference to London was that being in the great centre of the nation he might more rapidly spread his methods than if he stayed in Edinburgh. And thus it became entirely a question of how to spread his methods most quickly, for his heart was with suffering humanity. The problem was whether it was best to turn out a large number of followers every year from Edinburgh or to occupy a more central position in London. His decision ultimately was that as he would leave behind him in Edinburgh men who could carry on his work, matters would probably progress more quickly if he went to London. There is no doubt that it was a very great wrench for him and his wife to tear themselves away from Edinburgh, and there were times when he doubted very much the wisdom of his choice.

Lister's action in deciding to leave Edinburgh and go to London is most characteristic of the man. His heart was in Edinburgh; he had spent many years of his life there, his father was dead and he had not any very close ties in London. His position in Edinburgh was unique. He was revered by his pupils and by many friends, but when, as he thought, humanity called him he did not hesitate to place the call of duty above the call of pleasure.

He arranged to begin his work in London at the beginning of the winter session in October 1877, and he was asked to give the address at the opening of the winter session. At that time he had become very interested in the question as to whether there were different 'species' of bacteria each with its own actions whether on fermentescible substances or in connection with diseases, and he looked on this as a very important matter. He had already been working with milk, and he decided to investigate the lactic fermentation of milk, which he did during the summer and autumn of 1877. He satisfied himself by many ingenious experiments that a small bacillus, to which he gave the name of *Bacillus lactis*, was the only cause of the lactic fermentation of milk, and that that was its sole external function in the scheme of nature. The research was carried out with the precision and thoroughness which characterised all Lister's work.

He not unnaturally thought that the absolute demonstration of a specific bacterium, explaining much that was obscure at that time, would be of the highest interest to his audience. Large numbers of the leading surgeons in London and the provinces came to hear this lecture. Perhaps not unnaturally they expected to be told about the revolution in surgery which Lister had inaugurated, and instead they had to sit for an hour listening to details of a series of experiments which proved that the lactic fermentation of milk was due to a particular bacterium! The expression on the faces of the audience was very interesting and rather amusing; the majority of the surgeons present could not understand what the lactic fermentation of milk had to do with surgery. Those were the days

of the 'practical surgeon' as opposed to the 'scientific surgeon,' the latter being supposed (why I can't imagine) to have no practical skill or knowledge. It took a considerable time before the 'practical surgeon' realised that his day was past.

When we arrived in London, as far as I can remember no one at King's College was doing anything specially in the direction of antiseptic work, and, indeed, only a very few surgeons in London. As regards Lister's lectures, the chief thing that struck us at once was the almost entire want of enthusiasm among the students as compared with Edinburgh; there was no crowd running to get a good seat. Apart from Lister's dressers only a few students would stroll into the theatre quite casually, apparently taking little interest in the lecture and seldom taking any notes. After the enormous classes in Edinburgh and their great enthusiasm it was very disappointing and depressing. We found after a time that probably the main cause of this was that the examiners in surgery for the various diplomas and degrees were for the most part not yet favourable to Lister's views, and the students were therefore afraid to attend Lister's lectures in case they should by accident give his views to the examiner. A good many of them, however, planned to stay in town for some time after they qualified so as to see something of Lister's work. This state of matters continued for some time, but gradually died away.

Lister was quite well received by the staff and the students, but the nursing department was very difficult to work with. The hospital was at that time nursed

by a sisterhood who looked on the wards as their private rooms, into which no man should come without their permission, nor should he interfere with any of their arrangements, such as ventilation, etc. They could always produce a rule of their sisterhood why we should not do this or the other thing which we considered essential for the success of our work. Had we not possessed a keen sense of humour (being more or less of Scottish descent) I do not know how we could have got through, but most of the restrictions were so childish and so evidently invented on the spur of the moment that we could not but laugh at them. I believe the real trouble was that till we appeared on the scene the nurses dressed the wounds, but when Lister arrived he naturally would not allow them to meddle with the dressings; this solution did not occur to me for some considerable time.

The staff, on the other hand, were quite cordial and interested. Not long after we arrived one of the surgeons was going to amputate a limb, and he asked me to come and look after the antiseptic arrangements. His technique was pretty defective, and he did not seem able to visualise the germs which were ready to seize every opportunity of getting into the wound, so I accidentally from time to time squeezed a quantity of carbolic lotion over his hands, for which I was, of course, very apologetic. He did everything that I suggested till it came to tying the vessels, for which he used silk and left the ends long. I begged him repeatedly to cut them short and told him that they would never separate if the wound remained aseptic, but he was adamant: he had never seen a case where the ligature could not be pulled out after a few days,

and he was not going to carry out such stupid advice as to cut them short. The wound healed by first intention, except where the bunch of ligatures was, and no suppuration occurred. Day after day I dressed the case for him, and he pulled the threads without success. Gradually he began to think that I had told him the truth, and ultimately I got him to pull the threads as far down as he could and cut them off as high up as possible. This he did, and fortunately, as I had taken special pains to keep everything aseptic, no infection took place, and that end of the wound was healed in a few days and no further trouble resulted.

At the time of the migration to London we were still using carbolic gauze, carbolic spray, and carbolic lotions, and although thymol, eucalyptus, and various other antiseptics were tried during the next four or five years no real progress was made in the way of substitution of antiseptics till after the publication of Koch's work on antiseptics. After that Lister took up various mercurial preparations, and a variety of gauzes impregnated with preparations of mercury were employed which had all to be discarded after a time on account of irritation of the skin. Ultimately Lister examined a double cyanide of mercury and zinc, and this was found to be unirritating, being insoluble in water. But it was effective as an agent to inhibit the development of bacteria, though possibly not to destroy them. It was soluble in the mixture of serum and blood which came from the wound in sufficient amount to render it an unfit soil for the growth of bacteria. This we have used ever since with quite satisfactory results, and I believe that it just fills up

the loophole which is left where dressings which are simply sterilised but do not contain any antiseptic are employed. Where a large amount of discharge is expected, I generally reinforce the cyanide dressing by a mass of salicylic wool placed outside it. As to these gauzes and wool, it must be remembered that though they contain antiseptics they must be sterilised in the autoclave also, because the antiseptics are not volatile, so that bacteria which may lodge in the dressing are not acted on by the antiseptic. The diluted lotions of bichloride of mercury and of biniodide of mercury (1–2000 and 1–4000) were also largely substituted for carbolic acid, the carbolic spray having been given up in 1887.

Lister's active work in connection with surgery came to an end in the fateful year 1893. The death of Lady Lister in the spring of that year, followed soon afterwards by Lister's retirement from the hospital on account of the age limit, removing at the same time his much-beloved companion and fellow-worker for many years and the opportunity of pursuing his work at the hospital, broke him down and changed entirely the current of his life. But by that time general septic diseases had become practically unknown in his practice in so far as operations through unbroken skin were concerned, and suppuration, even of the mildest type, was an occurrence of extreme rarity, and the possibility of septic complications was quite negligible. He had also reached a point in his technique where the possible irritation caused by the antiseptics employed was not noticeable and did not interfere at all with the healing of the wounds.

In 1895 Lister was elected President of the Royal Society, and the work connected with that Society gave him another five years of life. But after that, although honours continued to be showered on him, the great brain gradually became tired, his memory failed, and he began to long for the time to come when, as he hoped and firmly believed, he would meet his wife and his friends again and commence a new life.

LISTER'S ACHIEVEMENT

APPENDIX

In order to realise the enormous value of Lister's work one must try to put oneself back into the conditions of sixty or seventy years ago and fancy that one is living in a time when very few surgical operations are performed and when the mortality after operations or wounds is very great.

When a wound was made either accidentally (*e.g.* compound fracture) or by the surgeon (*e.g.* amputations), the great majority of the cases developed fever (irritative fever), which lasted three or four days, and then suppuration took place, and when that was fully established the fever gradually subsided and, in favourable circumstances, the patient recovered. But a large proportion of cases, varying no doubt with the surroundings, the treatment, and the resisting power of the patient, the nature of the injury and other factors, instead of improving, developed, after a few days, a number of very serious ailments, *e.g.* septicæmia, pyæmia, erysipelas, various forms of gangrene and tetanus, and ultimately the patient died after much suffering. This series of events was so frequent in some hospitals that no operations were performed except such as were immediately required to prevent otherwise certain death, and the range of operations in general was,

therefore, quite small as compared with what it is at the present time.

When I was Lister's house surgeon in London there was a recess in my bedroom with shelves in it on which were kept a number of notebooks of the surgical wards. If I were sleepless or awoke too early I used to take down these volumes and read them. The notes of the cases commenced with a careful history of the illness and a description of the disease or injury. In cases where operations were performed an elaborate account of the procedure was given. And then in an extraordinarily large number of the cases this was followed by a statement that the patients had died of one or other form of septic disease; healing by first intention seemed to have been quite a rare occurrence. To one trained in Lister's methods and in his wards these books gave an appalling picture of the surgical results of the period to which they referred, then only a short time before Lister went to London.

This state of matters had lasted for centuries and had come to be looked upon as the natural and inevitable course of events after injury or operation. Much had been written as to the cause of these troubles, and the best way of dealing with them, but without arriving at any unanimous agreement. Usually it was attributed to the air ; to its temperature (too hot or too cold), to the oxygen of the air, to miasms in the air, to unhealthy surroundings, to crowding of people together, especially in hospitals, and to unhealthy conditions of the patient. About the time that Lister commenced work the term 'Hospitalism' had come into use and a good deal was being written about it. Sir James Young Simpson, a man of

extraordinary ability and versatility—the introducer of chloroform and acupressure—was a great advocate of this view, and the feeling, not only in this country but abroad, was that the infected hospitals should be pulled down and reconstructed on other principles: for example, on the pavilion system. Lister's work, however, coming soon after, rendered this drastic procedure unnecessary.

Naturally in a case like this, where there was no agreement as to causation, there was no agreement as to treatment, and an extraordinary number of methods of treating wounds were introduced. I need not go into them here: in my book on 'Antiseptic Surgery,'[1] published in 1882, I noted most of the methods and views, and reference to it will give some idea of the jumble of methods and opinions which existed when Lister began his work. Among the methods may be mentioned invocations, salves, lotions, exclusion of air by masses of dressings, open treatment, irrigation, water bath, water dressing, poultices, substitution of other gases for oxygen, and even antiseptics of various kinds.

It was to this bewildering state of matters that Lister, while a student, was introduced when he reached his surgical studies, and so shocked and impressed was he by the mortality and suffering after operations and injuries that he felt that nothing in medicinal work so much deserved attention as the problem of septic disease. After completing his studies at University College, London, he was strongly advised to go to Edinburgh and follow the clinic of

[1] 'Antiseptic Surgery: its History, Principles, Practice and Results.' By W. Watson Cheyne. Smith, Elder & Co., 1882.

Mr. Syme, who was not only looked on as one of the greatest surgeons of the time, but also was said to get better results in the treatment of his wounds than were obtained by the majority of surgeons of that day.

When Lister settled down in Edinburgh after having acted for a year as house surgeon to Mr. Syme, he took up the subject of inflammation, and produced his classic work on the early stages of inflammation and the *modus operandi* of irritants, and later did a good deal of work in connection with coagulation of the blood which proved most useful to him afterwards. After much study of the whole subject, he ultimately came to the conclusion that these septic diseases were closely connected with the putrefaction of the blood and serum in the wounds and the absorption of the products of this putrefaction, for when wounds healed by first intention there was on the one hand no putrefaction and on the other no septic troubles. He felt that if he could prevent this putrefaction he might get rid of these septic diseases.

On enquiring further into the causes of putrefaction Lister did not find any agreement, but the general view seemed to be that it was due to the action of the gases of the air, most probably the oxygen. But Lister was very much impressed by an observation by John Hunter, namely, that though as the result of the fracture of a rib the lung might be punctured and air escape sometimes in large quantity into the pleural cavity and the subcutaneous tissues, there was not necessarily any inflammation or putrefaction or septic disease. Lister had also made a similar observation himself and was satisfied that it could not be the oxygen of the air alone which was the cause of the putrefaction.

Before Lister's time other surgeons had also expressed very much the same views as regards putrefaction and septic disease, and attempts were made in various ways, *e.g.* by constant irrigation, water bath (medicated or not), and during the fifth decade of last century by the use of various tar products, to prevent or at any rate diminish this putrefaction. Although considerable improvement followed some of these methods no striking advance was made, and matters in the early 'sixties remained very much in the condition in which they were when Lister was a student.

Another suggestion, to the effect that the infection of the patient, and possibly also the putrefaction in the wound, were due to indefinite substances in the air to which the name of 'miasms' was given, did not help matters, because although no one exactly knew what a 'miasm' was it was also assumed to be gaseous. On either view the surgeon was up against an apparently insoluble problem. How could he prevent the air, or, indeed, any gaseous substance, from gaining access to the wounds? He could not operate or dress the wounds in a vacuum nor in an atmosphere deprived of oxygen. Any radical treatment seemed to be impossible, and there appeared to be nothing for it but to try to palliate matters as far as possible.

The most evident thing to begin with was to enforce strict cleanliness in dealing with wounds. At that time, in dressing a wound, the hands and instruments became inevitably soiled by putrid pus, and it was not the custom to wash the hands or instruments after every dressing; wiping the hands, etc., with a dry towel was considered sufficient, followed by a final good wash at the end of the visit. Lister, however,

introduced an era of cleanliness such as was unknown before, and which amused his colleagues and staff, and by some was even looked on as a profane attempt to fight against the decrees of the Almighty. Piles of clean towels were put in the wards, and plenty of water and basins, and those engaged in dressing the cases had to wash their hands frequently. He also used kettles of boiled water, often mixed with Condy's Fluid, with which he tried to wash out the putrefying discharges, and, in addition, Lister administered sulphite of soda as recommended by Polli. There was, however, no appreciable gain from these methods, and we can easily imagine Lister's state of mind, pretty sure that the key to the matter lay in the putrefaction of the discharges, and yet unable to attack the cause of the septic diseases with any chance of success on the view that he was dealing with a gaseous enemy. One can picture his despair when he saw that the best methods which he could think of did not help him at all, and that pyæmia and all the other septic troubles dogged his footsteps and rendered his best work nugatory.

I have discussed in the lecture at sufficient length the change in the aspect of affairs which the publication of Pasteur's papers on fermentation and spontaneous generation produced, and the suggested methods of applying this knowledge by the three surgeons who realised its value.

In the lecture I have also mentioned Lister's decision to select for his first experiments cases of compound fractures and have given his reasons for that decision. Lastly I have mentioned his reason for choosing chemical disinfectants and especially carbolic acid.

We have now reached the period when he was waiting for suitable cases to turn up. While waiting he no doubt thought much over his plans of campaign and repeated Pasteur's experiments, with additions, so that he was quite ready when the first case of compound fracture turned up.

His first case came into hospital in March 1865, but evidently was a failure. Lister says that 'it proved unsuccessful in consequence, as I now believe, of improper management.'

He did not get another case of compound fracture for five months (August 12, 1865), and then he had a compound fracture of the leg from the wheel of an empty cart passing over it. The wound was quite a small one, 1½ inches long by ¾ inch broad. In this case a piece of lint was dipped in undiluted carbolic acid and laid on the wound and overlapping the skin by about ¼ of an inch all around. Splints were applied and the leg left alone for four days; when examined after that time he found that there was no suppuration, but where the lint overlapped the skin it had caused a superficial slough. The wound was then dressed with water containing a little carbolic acid, and after further five days, as the deeper part of the wound seemed completely closed, he used water dressing, and the case healed without delay.

A month later he had his third case, also a fracture of the leg, from the kick of a horse, where the external wound was small. His house surgeon, according to directions, put the leg up in pasteboard splints, having laid a piece of lint soaked in the undiluted acid over the wound, overlapping the sound skin by about a quarter of an inch in every direction. He changed this

dressing in the evening and put on a fresh piece of lint similarly soaked, and outside that a piece of oiled paper to keep in the carbolic acid vapour. This lint was not disturbed again for some time, but a little carbolic acid was rubbed on the outside of the oiled paper every day. On the seventh day the skin at the edge of the wound showed signs of irritation from the carbolic acid and the dressing was left untouched for four days. On the eleventh day Lister removed the crust and found 'a concave surface destitute of granulations and free from suppuration.' He then thought that the case had made such progress that he might leave off the carbolic acid, which was irritating the edges of the wound, and substitute water dressing for it, and by the sixteenth day the little granulating sore presented a healthy aspect except at one point where a small piece of bone was bare. He then left Glasgow for some weeks and looked on the case as out of danger. But on his return he found that hospital gangrene had attacked the wound, and ultimately amputation became necessary to save the patient's life. The occurrence of phagedæna at that stage is very remarkable, and emphasises the value of the antiseptic, for it was not until he left off the carbolic acid and substituted water dressing that the phagedæna appeared.

His fourth case was a more severe one, also a fracture of the leg, where there was great distension of the limb with blood, and air had also been sucked in. The treatment here was much the same, but the carbolic acid was thoroughly mixed with the blood, and the result was perfect. A thick crust of blood clot, carbolic acid, and lint was formed. The blood clot and lint remained adherent to the wound for three weeks,

when, as Lister was trying to cut away some of the lint, he opened a cavity in the clot, which bled, showing that the clot was nearly completely organised in spite of the carbolic acid, and that new blood-vessels had formed. Those who have such a horror of carbolic acid and other antiseptics might well ponder over this fact.

The next case was a much more severe one, the patient's arm having been caught by the strap of a machine, and the tissues severely lacerated and the ulna fractured. Here Lister had to saw off the protruding portion of the ulna and remove tags of muscle, etc. In this case he applied the undiluted acid very freely to the whole wound, leaving a clot formed of blood and carbolic acid. On the sixth and seventh days (the crust having been daily painted with carbolic acid) some muddy fluid was observed at one part of the clot, and was attributed, probably quite correctly, to the irritation of the carbolic acid, which had been applied in excess of requirements. The wound went on well, and entirely healed in less than seven weeks.

I will quote only one more case. (It is worth while to read Lister's first paper, especially as it is so typical of his patience and careful observation.) This case was one of very bad compound fracture of the leg with a very extensive wound, and it was treated in the same way as the above. Carbolic acid was extensively applied, and the patient gradually recovered from the severity of the injury. At the end of a fortnight he was doing very well, but it was then found that a small wound on the outer side of the leg had been overlooked and had not been treated by the carbolic acid, and that,

while the other parts were going on well, this sore was not satisfactory. To put it shortly, phagedæna appeared in that wound, spread rapidly to the main wound, and it became a prolonged fight against the phagedæna, which, however, ended in the complete recovery of the patient after about nine months in the hospital.

These cases of phagedæna, especially the last, are remarkable as showing how infected the wards must have been, and it is not wonderful that Lister, with such experiences, acquired a great faith in the value of carbolic acid as an antiseptic.

Very interesting results were obtained in these cases as regards the behaviour of blood clot in an aseptic wound, and the former ideas on the subject were completely upset. The former experience was that if blood clot formed in a wound it very quickly putrefied and became liquid, and was washed away with the pus. Lister's very first case showed him a different phenomenon as regards blood clot when mixed with carbolic acid. Instead of putrefying and becoming liquid, it remained intact and adherent to the tissues of the wound. By and by it became infiltrated with young connective-tissue cells from the surrounding parts and these developed into connective tissue. After a time epithelial cells spread over the surface of the clot, or rather, beneath a thin layer of clot left on the surface, and so a cicatrix was formed without necessarily any granulation or suppuration. This aseptic clot also did not contract and squeeze out serum as it

does if shed into an ordinary vessel, and so it filled up considerable cavities. This is a most important point of which advantage has often been taken, viz. if a cavity must be left in bone or in the tissues and if there is not enough blood clot to fill the cavity, it is well to scratch the sides and make them bleed a little more, so as to leave a complete mould of blood clot into which connective-tissue cells can migrate and develop into fibrous tissue and so close the cavity.

I think that the significance of these facts is not sufficiently appreciated at the present time. Blood clot in a wound is not an obstacle to healing. It would not be there if that part of the wound were not aseptic; it would have liquefied and disappeared. It might, no doubt, prevent the escape of discharge from the deeper part of the wound, but it is quite easy to push a drainage tube in through it or, better still, to make an opening at another place into the recess which needs drainage. To tear away adherent clot under the impression that it is a bad thing is to make one wonder whether the surgeon who does so is really accustomed to aseptic results. Lister left the blood clot alone in his early cases, merely painting the surface of it from time to time with a little undiluted carbolic acid.

We now come to the next step. Lister, having satisfied himself by this time that he was following the right lines, proceeded to consider how *to apply the principle to operation wounds.* In the meantime the chemists had been at work and had succeeded in getting a solution of carbolic acid in oil up to a strength

of one part of carbolic acid to three parts of boiled linseed oil, and they were also making progress with the solubility of the acid in water. After much consideration Lister elaborated the essential conditions for success in operation wounds through unbroken skin as follows :

(1) The first thing that has to be done is to destroy the germs on the patient's skin, on the surgeon's hands, on the instruments which are to be used and on everything surrounding the area of operation.

(2) The second is to prevent living germs from entering the wound from the air or the surrounding objects during the performance of the operation.

(3) And the third is to prevent germs from spreading into the wound after the operation.

These three principles still hold the field whatever methods are employed to carry them out, and they are not likely to be given up till some means have been devised for protecting the body against all germs which may enter it, and this is certainly not within sight at the present time.

Lister's first *operation* was the opening of a psoas abscess, and it is interesting to note how he did it.[1] He first prepared a mixture of one part of carbolic acid to three parts of boiled linseed oil. A piece of lint about six inches square was soaked in this solution and placed over the site of operation for some time before it was performed ; the instruments were also placed in carbolic oil, and naturally the surgeon's and assistants' fingers became coated with it. When everything was ready the piece of lint was raised at

[1] I cannot find the exact date of this operation, but I think that it was early in 1867.

one side and an incision was quickly made into the abscess ; the lint was then allowed to fall back over the wound and the pus was squeezed out of the abscess under cover of the oiled lint, and finally a strip of the oiled lint was pushed through the opening into the abscess in order to keep the wound from closing. I have been told that on this occasion Lister made a paste of the pus as it escaped from the abscess and carbolic acid, just as he had done with the blood in compound fractures, and he used this paste as an antiseptic dressing. Would those who have no faith in antiseptics try to visualise this ? This cheesy pus contained caseating tissue and no doubt many tubercle bacilli or their spores, and yet, mixing it with carbolic acid, he used it as an antiseptic dressing ! Next day, on proceeding to dress the wound and intending to make a fresh paste with the pus which he expected to squeeze out of the wound, he found to his astonishment that he could not squeeze out any pus at all but that only two or three drops of clear serum escaped. He was for the moment nonplussed what to do for a dressing under the circumstance, when it was suggested that if there were any whitening in the hospital he could easily make a putty with the carbolic oil. This was done and the putty was applied over the wound with a piece of block-tin outside it and fastened down with strapping. In this case the wound remained aseptic, but it was some months before it had completely healed. This putty turned out to be a useful dressing and was employed for a considerable time, but it was very apt to crack and crumble, and so Lister set to work and ultimately replaced it by ' lac plaster.' Nevertheless, the putty served its purpose and showed that operations

could be performed with safety by the antiseptic system which other surgeons, using the older methods, looked on as quite unjustifiable on account of the septic dangers.

A case in point is given in Lister's collected works, volume ii, p. 72. This was the case of a patient who had met with an accident some time previously and had sustained a fracture-dislocation of the ankle joint. In this case union of the bones had occurred in bad position, with the result that the patient was entirely crippled. He was admitted into the hospital in April 1868, and operated on on April 11, about three years after Lister's first unsuccessful case. Lister cut down on the fracture under the protection of carbolic oil, divided the bones at the seat of fracture, of course opening the ankle joint, and brought the bones into good position. The wound was covered by layers of lint soaked in weak carbolic oil and covered with a plaster consisting essentially of emplastrum plumbi with some beeswax and crystallised carbolic acid (about 8 per cent.). The ultimate result was complete restoration of function.[1]

[1] Evidently at this time (1868–69) Lister did occasionally have suppuration in some of his wounds, which he attributed either to tension or to the irritation of the carbolic acid, but which can hardly have been severe or accompanied by serious symptoms. For in one of his papers at this time he points out that in the case of the dressings which were being used at that time (viz. the application of two or three strips of lint dipped in weak carbolic oil and then covered by the lead plaster containing carbolic acid) the carbolic acid soon disappeared from the deeper layers of lint, and that it would be best to change the dressings every day, *even though that might involve suppuration* from the irritation of the carbolic acid, rather than run the risk of putrefaction in the deeper parts of the wound; 'the object' (of the antiseptic treatment) 'being not the avoidance of suppuration, but the prevention of putrefaction in the wound.' See Lister's Collected Papers, vol. ii, p. 78.

But although good results were being obtained at this time, it was evident that the arrangements, dressings, etc., were so elaborate and troublesome that while they would, of course, be carried out by Lister and his staff, in lieu of anything better, they would not appeal to the surgical profession generally as practical propositions, and this would be more especially the case as very few surgeons had as yet accepted the principle on which Lister acted : without belief in the principle the result would be disastrous. Lister therefore set to work to simplify and improve matters in every way possible. He put as his aim that an operation wound should heal as readily and completely and as free from general disturbance as the same injury would do if the skin were intact and had never been incised. In fact the operation must not involve more irritation than a subcutaneous wound would, and the dressings must serve as an efficient substitute for skin, painless and non-irritating, and form as complete a protection against infection.

In fact this was not an impossible aim, for Stromeyer and others had already introduced subcutaneous surgery, and had shown that, if the hole in the skin were tiny enough and if the knife were clean, division of tendons and muscles and bands of fibrous tissue could be carried out extensively without local or general infection and without any marked local disturbance. This was Lister's chief aim throughout the rest of his active life and explains his constant trial of new antiseptics, of different dressings, and all the other details which he was frequently altering with the view of improving the technique without impairing the efficiency of the treatment. Anyone who has time

and the wish to study the details of his work will find them in his collected papers, which are most delightful and instructive reading and are a notable example of the untiring investigator steadily pursuing the search after perfection, open-minded, ready to acknowledge error and to take up and investigate any suggestion which might promise further progress.

By this time the chemists were able to get a much more refined carbolic acid which was found to be soluble in twenty parts of water and could be diluted considerably before losing its antiseptic power. This enabled Lister to get rid of the carbolic oil which, though it succeeded in the case referred to above and in others, was quite unsuitable for general use, as it obscured the field of vision, and thus, apart from the question of irritation, made the performance of a delicate operation impossible. The possibility, however, of obtaining watery solutions of carbolic acid led to a rapid extension of his methods, and he adopted the following procedure.

Before the operation the skin of the patient, and also the surgeon's and assistants' hands, were well washed with the 1–20 carbolic lotion, and to prevent the infection from the air some carbolic lotion (1–20 or 1–40) was poured over or syringed into the wound from time to time during the operation. The instruments were placed in a dish of 1–20 carbolic lotion for about an hour before the operation, and basins of 1–40 carbolic lotion were at hand during the operation to wash sponges or rinse the fingers in from time

to time. About the same time Lister introduced the ' lac plaster ' which held the field as a dressing for about two years.[1]

Marine sponges, ligatures, etc., were kept in jars of 1–20 carbolic acid till they were required.

It was about the end of 1867 that Lister took up the subject of ligatures. Before the antiseptic period the silk ligatures were left long, the ends of the threads coming out in a bunch at one part of the wound. These ligatures in course of time became loose as the result of ulceration through the walls of the vessels and they could then be pulled out of the wound. From the point of view of the antiseptic treatment there were two objections to this procedure. In the first place these ligatures passed down to the depths of the wound, and should any infective material reach the surface of the wound it would rapidly spread down to the interior along the ligatures. On the other hand, if the wound remained aseptic the ligatures would not ulcerate through the vessels and become loose, and it would be necessary later on to open up the track and cut them short. Lister, therefore, saw that in aseptic work ligatures must be cut short at the time of the operation, and the question was, was silk the best material ? He tested this on a horse by tying the carotid artery with silk which had been soaked for a considerable time in

[1] This plaster consisted of shell lac three parts and crystallised carbolic acid one part, melted and thoroughly mixed, strained through muslin and spread to the thickness of $\frac{1}{30}$ of an inch on linen. When cool, the surface of the plaster was brushed with a solution of gutta-percha in thirty parts of bisulphide of carbon to prevent the stickiness.

1–20 carbolic lotion. The wound healed by first intention, and when he examined the part six weeks later he found the silk around the vessel apparently unaltered but buried in dense fibrous tissue.

Some weeks later he tied the external iliac artery in a lady, suffering from aneurism of the femoral artery, with silk, which he cut short. The wound healed by first intention and the patient recovered, but died suddenly ten months later from rupture of an aneurism of the aorta. Lister thus had an opportunity of examining the seat of his ligature. The noose of the ligature had disappeared altogether, evidently eaten up by the tissues, but the knot and two tapering ends were found in a little cavity containing a tiny drop of fluid which looked like pus. This fluid actually contained a few leucocytes, some ' fibroplastic ' corpuscles, and some short eroded fragments of silk fibre.

Although Lister was disappointed that the silk in the above case had not completely disappeared, I should have thought that the result was very satisfactory. However, Lister wanted to get a quicker disappearance of the ligature and proceeded to test catgut, and worked on this question of ligatures for many years. During that time catgut prepared by whatever method he was testing at the time was always employed. He laid down several requirements for suitable catgut which may be mentioned here.

(1) It must be strong enough after soaking in lotion to bear any reasonable strain required to occlude the vessel around which it is tied.

(2) The knot must hold for several days in the wound till the vessel has become thoroughly occluded.

(3) The catgut should not be too rigid, otherwise it may mechanically irritate the tissues.

(4) It must not be absorbed too soon (it should last from one to three weeks, according to circumstances).

(5) It must be sterile.

I need not go into the methods adopted by Lister to secure these objects, as there are several papers in his collected works in which he gives full details and which can be referred to by anyone interested in the subject.

Lister worked a great deal at catgut, nearly up to the end of his active life, in order to get a method of preparation which would fulfil all the requirements mentioned above. And after much work he ultimately adopted a sulphochromic catgut (see collected papers, vol. ii, p. 119), which he looked on as the best he could get and which for some years was quite satisfactory. Lately, however, it has not been doing so well (I think from about the beginning of the war). It is now very stiff and hard, and buried ligatures or stitches of this material are very apt to find their way out after a time. Not that there is any sepsis. What happens is that the wound heals quite satisfactorily, but if the stitches are superficial—say that two or three stitches have been put into the external oblique tendon after an appendicectomy—the hard stitches may be felt under the skin and the patient complains that they prick when touched. In course of time (some weeks usually) the patient returns with a red soft spot in the scar corresponding to the stitch, and on making a small incision a little patch of soft granulation tissue is found

and the stitch can be lifted out as rigid and firm as when it was put in. This has latterly become such a nuisance that I now use very fine silk for these superficial buried sutures in preference to the catgut, and no trouble results. I have still used the catgut for deeper stitches, especially when buried in muscle, and this more vascular tissue seems to be able to absorb it much more quickly. It is evident that something has gone wrong with the preparation of this catgut, probably in the way of over-preparation, and it would be as well to ascertain whether the original method is still being strictly adhered to or whether some 'improvement' has been introduced. So long as we got catgut made under Lister's directions we could rely on it, but since then so many methods of preparation have been introduced, some of which do not meet Lister's requirements, that I now prefer fine silk to catgut. Silk is more easily sterilised and its strength varies with its thickness (which is not necessarily the case with catgut), so that one can select the finest silk that will bear the strain that has to be put on it.

During this time, which I may call the actively constructive period of antiseptic surgery, Lister not only became more and more confident that he was working on the right lines, but he also found that there were certain points to which he must attend if he was to make the method generally acceptable. One was the irritation of the wound and surrounding skin by the antiseptic used (at this early period 1–20 or 1–40 carbolic lotion and carbolic gauzes). And another

was the occasional occurrence of symptoms of absorption of carbolic acid.

The irritation of the wound showed itself by the pouring out of a considerable amount of serum from the wound for twenty-four to forty-eight hours after the operation. At this early period in the history of antiseptic surgery irrigation of the wound with 1–40 watery solution of carbolic acid during the operation was practised, with the view of destroying the living germs floating in the air as they fell into the wound, and it was in order to get rid of this irrigation, which meant the introduction into the wound of a considerable amount of carbolic lotion, the obscuring of vision, the resulting irritation of the wound, and occasional carboluria, that Lister introduced the *carbolic spray* about the end of 1870. The spray continued in use till 1887, although from 1881 its value as a means of killing bacteria in the air was recognised to be very doubtful. Lister thought that by the use of the spray he would greatly reduce the amount of carbolic acid which came in contact with the wound and thus diminish the irritation and nevertheless kill the germs before they were able to develop in the wound. (The strength of the spray was from 1–30 to 1–40 watery solution of carbolic acid.)

This destruction of the germs by the spray was an assumption on Lister's part without experimental proof, a thing that he was very seldom guilty of; but there is this to be said, that no one was carrying on bacteriological research by cultivations or otherwise

at that time—in fact, bacteriology had not yet begun—and he had not the means of testing the point except clinically, and the clinical result seemed to be definitely in favour of his views. In fact, when I came to test it ten years later I found it no easy thing to do.

The serious part of the irritation of the wound by the carbolic acid was not that it caused any great delay in healing, for if the surfaces of the wound were brought into contact healing occurred by first intention without any sign of inflammation or suppuration. The trouble was that the carbolic acid caused the pouring out of a considerable quantity of serum, and if the edges of the skin incision were brought closely together the serum was apt to be confined in the deepest part of the wound and form a cyst which had to be opened and drained separately. Curiously enough, the fluid so formed did not always become absorbed, the lymph and blood clot covering the raw surface apparently preventing the serum from getting into the lymphatic vessels. In some cases it sufficed to apply pressure over the deeper part of the wound, but this was by no means always successful. Attempts were also made to bring the deeper parts of large wounds into contact by buried catgut stitches, but this could not always be successfully done. It became clear, therefore, that in large wounds it saved time to arrange from the first for the escape of the serum, and the only satisfactory way of doing this was found to be by the use of rubber tubes passed down to the deepest part of the wound and cut flush at the outer end with the surface of the skin, and so secured that they could not slip in. Lister's hope that the carbolic spray would irritate less than the irrigation was not established; there was still too much

serum from large wounds, especially if the deep parts could not come together, to allow the surgeon to omit drainage. At first Lister tried to drain the cavity by inserting pieces of *sterilised lint or gauze*, but, apart from the fact that bacteria were very apt to spread in through the gauze and so set up infection after the operation—and this is more likely to happen with gauze than with an open tube—it was a very imperfect drain, and after the first day the coagulation of lymph in the interstices of the gauze tended to prevent the fluid from escaping; hence this plan was given up. *Absorbable drains* were also tried, in the hope that it would not be necessary to handle a tube at all. A number of strands of catgut were twisted together and one end of the bundle was pushed into the cavity while the other end hung out of the wound. It was hoped that by the time the catgut became absorbed no further drainage would be necessary. But it turned out that, though the drainage went on well for a few hours, lymph and blood clot blocked up the capillary spaces between the threads before the flow of serum had ceased. Further, it was found that the catgut often became absorbed where it passed through the skin, sometimes, indeed, before the need for drainage had passed off. Decalcified chicken bones were also used for a time, but in a day or two they collapse and the drainage ceases. Hence, after all, the most satisfactory method was the use of a *rubber tube*, and the surgeons had to learn how to use it without letting in infection. Unfortunately, at that period in the development of asepsis very few surgeons believed in the principles of the treatment or saw anything more in the method than, as they said, 'just to splash carbolic

acid about the wound.' The result, of course, was that their wounds frequently became infected, and this made them very dissatisfied with Lister's methods. It never occurred to them that the fault lay in the way in which they used these methods. This trouble was one great incentive to Lister to search for less irritating antiseptics than carbolic acid. I shall refer to the question of the drainage tube again.

Quite early in his surgical work Lister gave a good deal of attention to the *sutures* with which the edges of the skin were brought together. At the beginning he took up the use of silver wire, and subsequently of fishing gut and horsehair, these being promising because they were non-porous, so that serum could not soak into them and form a nidus in the pores in which germs could develop: and further, they were easily disinfected. Recently silk has come back into more common use, as it is readily disinfected by boiling and is softer than the others. Up till the antiseptic period, surgeons had been very chary about stitches, mainly because they were apt to interfere with the free escape of the discharges from the wound, and also because they led to ulceration and suppuration along their track. Here, again, Lister pointed out that the state of matters was altered by the absence of sepsis, that the drainage tubes prevented the retention of fluid in the wound, and that suppuration ought not to occur along the stitches if the wound were aseptic; on the other hand, healing was very much expedited by bringing the edges of the

wound together. In cases of extensive operations for cancer of the breast, for example—which, by the way, Lister was one of the first to advocate and to carry out very thoroughly—he used to employ button sutures (a sort of interrupted quilled suture) to draw the bases of the flaps together and so relieve tension on the edges. Several pieces of strong silver wire were passed through the base of each flap, and outside the skin they were attached to broad oval lead buttons and pulled tight so as to approximate the bases. The loose edges of the wound could then be brought together by a continuous suture without any serious tension. After these lead buttons had been in for about ten days they were taken out, and though the skin beneath them looked white from the pressure, generally most of it recovered, and if there were a small slough, no bad effects followed, because there was no sepsis, and the healing was much quicker and more satisfactory than without these button sutures. Lister used to speak of these deep sutures as stitches of relaxation, and the others as stitches of apposition.

In case there should be irritation from the dressing along the line of incision, Lister quite early introduced a material, which he called *protective*, to cover the line of incision. This material consisted of oiled silk covered with a layer of copal varnish, and when dry dipped in a solution of dextrine, the latter layer being put on so that the surface should be equally moistened by the antiseptic lotion before it was laid over the wound. This protective was impermeable to carbolic acid, and

after steeping for some time in the antiseptic solution a narrow strip was laid over the line of incision and was well overlapped on all sides by the antiseptic dressing.

The *dressings*, especially those containing carbolic acid, also sometimes irritated the skin, and that was one of the reasons for giving up the lac plaster. Lister was able to diminish this tendency to irritation a good deal by altering the composition of the antiseptic material. Glycerine or a mild antiseptic ointment kept well away from the line of incision sometimes put this right, but one of Lister's chief reasons for his long research after new chemical antiseptics was to find one which did not irritate the skin.

It was about this time that the first of the various antiseptic *gauzes* was introduced. Lister had found the lac plaster unsuitable in various ways. In the early days he packed unsterilised cloths outside the lac plaster in order to catch the discharge as it came through, it being presumed that that discharge in passing over the lac plaster would pick up enough carbolic acid to make it an unsuitable soil for the growth of bacteria; but it happened more than once that the lac plaster shifted during the movements of the patient and the unsterilised cloths came in contact with the wound. Further, there was a good deal of irritation of the skin under the lac plaster which was objectionable, and so Lister began to try other sorts of dressings.

Oakum had been suggested to him as a dressing, and he found that it had considerable advantages, and this led him to the use of *gauze*. The first gauze which he used was impregnated with a mixture of 16 parts of paraffin, 4 parts of resin, and 1 part of crystallised carbolic acid, but these proportions were

varied from time to time. The principle was that the resin should hold the carbolic acid, so that it should not be washed out immediately by the flow of serum but nevertheless should impart enough of the acid to the serum to render the latter an unsuitable soil for the growth of bacteria, while the paraffin did away with the stickiness and irritating qualities of the resin. In order to economise the gauze, Lister folded a large piece into eight layers, and inside the outer layer he placed a piece of mackintosh (which had been soaked in 1–20 carbolic acid) with the mackintosh side downwards, so that before the discharge reached the outer world it had to pass through what was equal to considerably more than seven layers of gauze.

I have also mentioned *absorption of carbolic acid.* This might go to the length of producing carboluria, and even hæmaturia. In children especially one had to be careful not to leave a compress of carbolic acid on the skin for any length of time, and this was also the case in people who were ill, especially with hectic fever. But I must say that with care there was extraordinarily little trouble in this respect.

This was the condition of affairs about the end of 1871. By that time the victory over sepsis, so far as operation wounds not opening into septic places were concerned, had been completely achieved. True, there were many ways in which the technique might be

simplified and improved, but even with the technique as it then was it was possible to rely on aseptic results and to feel certain that operation wounds through unbroken skin would not become septic. And never forget that *the victory over sepsis was obtained by the use of chemical antiseptics*. Whatever those who take a curiously exaggerated view of the disadvantages of chemical antiseptics may believe, this was the fact, and as time went on and improvements in technique occurred the avoidance of sepsis became more and more a certainty, and the only persons who suffered any disability from the antiseptic substances were the operator and his assistants, whose hands became rough and the fingers somewhat numb, especially after a long operation or a succession of operations.

While Lister continued his attempts to improve his technique, he was in the early 'seventies energetically engaged in revolutionising surgical practice, in view of the fact that, in so far as the danger of sepsis was concerned, most operations had now become quite safe, instead of being most dangerous, as was the case seven years previously. For some years Lister and his staff went on acquiring experience and dexterity in the methods employed but without any great alteration in those methods till the early 'eighties.[1]

I have referred in my lecture to the developments which were taking place in 1872, namely, those affecting the improvement of the technique and those

[1] As regards the things employed in treatment about 1873, when I became one of Lister's dressers, they were, as lotions, Carbolic lotion 1–20 for disinfection of skin and instruments, Carbolic lotion 1–40 for wounds, Boracic lotion and other antiseptics that he might be trying for ulcers, etc.; also Carbolic acid dressings, protective boracic lint, drainage tubes, catgut, silver wire, horsehair and silk. The hand spray also came into use about that time.

following from the fact that the victory over sepsis had been gained so far as operations through unbroken skin were concerned. This fact led Lister to reconsider the treatment of every surgical case which came before him from the point of view of whether a better result could be obtained by operative treatment than by the methods of treatment then in vogue. This revision and extension of the operative treatment to various ailments and injuries, now that he need not fear sepsis in the operation wound, went on very rapidly. I have already referred to the first case of badly united fracture at the ankle joint which he rectified by open operation, opening the ankle joint freely and without the occurrence of sepsis.

At first he did more bone work than anything else, ununited fractures, badly united fractures, deformities of bone (he was the first to operate for knock-knee, very much on the lines of the operation devised by Sir William Macewen some time later), wiring or pegging recent fractures, *e.g.* fractures of the patella or olecranon, operating for varicose veins, open radical cure of hernia, extensive operations for cancer of the breast, and so on.

He was the first to resuscitate suprapubic lithotomy, though this is generally attributed to others. In fact, his remarks on this matter will be found in my notebook, where you will see his reasons for this suggestion, but he was not very happy in their practical application. It is impossible to mention all the alterations in treatment and additional operations which he introduced as the result of freedom from sepsis : he never approached a case without thinking how the treatment might be affected and improved by the aseptic question.

The new surgical procedures which were introduced by him are too numerous to mention.

The other main lines of research which he followed were, firstly, the hunt for antiseptic substances which were as good as carbolic acid but did not possess its objectionable characteristics, and, secondly, the improvements of his technique in every direction. The latter was, however, not so urgent, for as his followers gained more and more experience in practice the cases of failure became fewer and fewer.

Many antiseptic substances were brought to Lister's notice and were subjected by him to the most stringent tests, both as regards antisepsis and also as regards the irritation of wound and skin. At this time bacteriology was practically non-existent, and scientific tests could not be made with the same precision as at the present day, but Lister subjected them to severe practical tests. He said, 'I want to know whether these chemical substances will irritate the skin or the wound, whether they will prevent or arrest putrefaction, how they will behave in serum and blood, which are my cultivating media (not in Pasteur's solution, or other artificial media), whether the gauze can store enough antiseptic to prevent growth of bacteria in the blood and serum which pass through it, and so on.' I need not go into details here as to his methods of testing the gauze, they are fully set out in his collected papers. I may, however, quote below an example of his mode of testing gauze impregnated with chemical substances.[1]

[1] 'I packed a piece of glass tube with gauze charged with three per cent. of the double salt (cyanide of mercury and zinc), and poured into it serum

Often when Lister took off his coat and turned up his shirt-sleeves preparatory to dressing a case (for the days of sterilised gowns had not yet come), one would see several patches of dressing over his arm stuck down with collodion or strapping, and would wonder what injuries he had sustained and how. Foreigners especially took a great interest in these patches. These were in reality pieces of gauze impregnated with various kinds and strengths of antiseptics which he had fastened to his arms so as to test whether or not they were irritating to the skin, and if so in what strength. If the tests were satisfactory he would test them next on a small, unimportant wound.

Lister, who believed everyone till he found out that his informant's powers of observation were not very reliable, used to accept the statements of others, and he was often led to think that he was on the verge of getting what he wanted in the way of an ideal antiseptic till he came to test the matter himself. I cannot remember how many times he came down to his class and told them of some new antiseptic which would displace the old irritating ones and read abstracts of papers or letters which had been sent to him. Nor can I remember what the substances were, but the great majority of them were found wanting in some respect or other, and had to be rejected. He rejected volatile antiseptics after a serious experience which he had with

and corpuscles obtained by whipping pigs' blood. I then inoculated one end of the saturated gauze with a drop of septic serum and kept it at the temperature of the body with provision for preventing evaporation. After the lapse of *five days* I found the entire mass of gauze pure in odour and without bacteric development, as tested by miscroscopic examination of stained coverglass preparations of the contained blood. Meanwhile, a piece of *unprepared* gauze similarly treated showed bacteric development within twenty-four hours.'

gauze impregnated with eucalyptus oil. He had several cases of suppuration and one case of death, evidently from sepsis, while using this gauze. On going into the matter he excluded everything except the eucalyptus gauze. On speaking to the manufacturer he found that the latter had deviated considerably from his instructions, in that after impregnating the gauze with the eucalyptus oil he had not put it away immediately in tin cases as he had been instructed to do, but had left the gauze lying exposed to the air in warm weather, with the result that the eucalyptus oil evaporated, and the gauze he was using was not even sterile, far less did it contain a powerful antiseptic.

Although Lister investigated many substances he did not find any so generally useful as carbolic acid, until after the publication of Koch's work on antiseptics in 1881 he was led to examine the salts of mercury. He found that some of these were, under certain circumstances, as useful and much less irritating than carbolic acid, and that being the case he restricted the use of carbolic acid greatly. He still used it to sterilise his instruments (1–20) and to immerse his instruments in during the operation (1–40), to disinfect the skin before the operation (1–20) and during the change of dressings, and to clean the skin around the drainage tube at each dressing so as to prevent bacteria spreading into the wound. The other antiseptic lotions employed were 1–2000 or 1–4000 solution of bichloride of mercury or of biniodide of mercury, introduced in 1882. They were used to dip fingers and instruments in during the operation, to wash sponges and generally to clean up with, but there was no douching of the wounds with this or any other antiseptic. The

antiseptic gauze finally used was a gauze containing the double cyanide of mercury and zinc, a salt which he found to be insoluble in water but sufficiently soluble in serum to inhibit the growth of bacteria, at any rate for a time.

We may now take up the bacteriological side of the question, on which comparatively little work had been done up till the middle of the 'seventies. True, Tyndall and others added to Pasteur's proof against spontaneous generation, and Lister himself repeated some of the experiments and introduced a very useful flask for storing fluid cultivating media in, and also pointed out the necessity for subjecting all flasks and tubes to a very high dry temperature so as to destroy all germs.[1] He did occasionally look at the discharges from wounds microscopically, but up to the time I am speaking of he did no definite bacteriological work with the exception of a paper read before the Royal Society of Edinburgh on bacteria, where, however, there is a possibility that contamination with other organisms had taken place. The results of his treatment were his test, and the success of it seemed to confirm his theories.

Up to this time (1876), with the exception of the suggestion as regards anthrax, there had not as yet been any definite evidence that bacteria entering into the body were the cause of specific diseases, but some tentative suggestions had been made as regards micrococci. Nevertheless, Lister was beginning to remark

1. See Lister's Collected Papers, vol. ii, p. 55.

that suppuration could occur without putrefactive odour in the pus, but he developed various theories apart from the growth of germs to explain what he still spoke of as 'aseptic pus.' (This view of 'aseptic pus' apart from micro-organisms was given up by him later on.) He was also beginning to suspect that erysipelas was different from the other septic diseases, and used to remark that it spread like fairy rings, the centre recovering while the redness, etc., spread at the circumference, as if (as he used to say) something were growing in the skin which rapidly exhausted its foodstuffs and so went on to a new place. We, however, never saw a really severe case of erysipelas, and those that we did see came into the hospital with the disease.

He was also much puzzled about tetanus. He was constantly remarking that, though tetanus was extremely common in pre-antiseptic days, he had never had a case since he started his antiseptic work. It certainly is very remarkable that, although in laboratory researches the organism of tetanus has spores which are very resistant, and although it takes a considerable time for a solution of carbolic acid to kill spores in cultivations, the disease should nevertheless entirely disappear when Lister began to swab out compound fractures with undiluted carbolic acid. Laboratory workers would say, I presume, that this could not be the case. They would assert that carbolic acid exerts a very deleterious action on the raw tissues and would destroy their resisting power to a very serious extent and thus open the door for infection, while it would be useless as a means of killing the organisms, and especially the spores. In this, as in a good many other

matters, the laboratory view does not necessarily agree with the clinical result. It is hardly conceivable that tetanus bacilli vanished from the soil about Glasgow when Lister began to use carbolic acid, and it certainly is not the case that Lister was making a mistake : there were too many observers about, and the fact was too striking. Is it at all conceivable that the laboratory workers may be wrong in thinking that carbolic acid is such an evil and useless thing as they say ? Whatever the explanation may be, the fact remains, and the real explanation might open up a very important new field of research.

There is one other point that I must just mention before passing on to other matters. Lister spoke a great deal in his lectures from time to time about the power which the healthy tissues and fluids of the body must possess in the way of killing germs, or at any rate of preventing their entrance into the body, and consequently he assumed that there must be some sort of protective arrangement. He pointed out that bacteria could not apparently spread along the ducts of glands such as the milk ducts, salivary ducts, etc., or along tubes lined with mucous membrane, *e.g.* the urethra, so long as the tissues are healthy. He published experiments where he had milked cows after thoroughly washing the udder and received the milk into sterilised tubes and flasks, and where a considerable number of the tubes remained free from any growth and without any alteration of the milk. From the very first he laid great stress on the protective arrangements

of the body and on the importance of interfering with them as little as possible. I shall refer to this matter again.

In October 1876 I became Lister's house surgeon at the Edinburgh Royal Infirmary. I may say that I missed a year after graduation because I had been too shy to ask Lister for his house surgeoncy, though I wanted it badly; and when he asked me in the summer of 1875 why I had not applied for his house surgeoncy he had already filled up the appointment for a year ahead, but he told me that if I wished it and could wait for a year I could have it later. Naturally I decided to wait the year if it were at all possible, and on going home I found that I could scrape together a small sum of money, and with this in my pocket I set off for Vienna in the autumn of 1875. In Vienna I attended a variety of classes during that winter (Billroth, Brücke, Politzer, Hebra, Exner, etc.), and in spring I went to Strasburg and worked for over three months in Von Recklinghausen's laboratory, so that the year was far from lost and my patrimony was well spent.

I may say that I was very much impressed with the enthusiasm that there was for Lister wherever I went. (The fact that I had been one of his pupils and would be house surgeon on my return to Scotland was an Open Sesame, and I was even asked in Strasburg to instruct the Professor of Surgery there in Lister's methods !) I could not but contrast this with the state of matters in our own country.

While abroad I had been asked all sorts of questions about Lister's work—Did he find bacteria in his wounds? How did we know that the germs were not present in the body in health and simply passed into the wound with the blood and serum? What proof had we that they were the cause of septic disease? and so on. Professor Lucke's assistant had already microscopically examined the discharge from the Professor's cases 'treated with carbolic acid,' and had seen all sorts of things swimming about in the fluid. Though I could not very well say it, I was not surprised.

On taking up Lister's resident appointment at the beginning of October 1876, I naturally told him of my experiences abroad, and asked him to allow me to examine the wounds in his wards and go into the various bacteriological questions which might arise. I saw that I had an opportunity which those with whom I had conversed abroad did not possess, namely, that my observations would be made on cases treated by Lister himself, and therefore would represent the actual bacteriological facts, and we could not plead, if the results proved unfavourable, that the treatment had not been thoroughly carried out. Lister was delighted and gave me leave to do anything I wished, and discussed plans with me and gave me much useful advice. It was, in fact, impossible for Lister to do this work himself, and therefore he was very glad to have it done under his eyes. The development of surgery as affected by the abolition of sepsis, the search for new antiseptics, the constant improvement of his technique, his duties as professor and surgeon to the hospital, which he took very

seriously, and his private surgical practice did not leave him any time to start on laborious bacteriological researches and to work at the various problems which very quickly confronted us.

Up to this time Lister's clinical work was based on Pasteur's investigations, and though he had done a certain amount of bacteriological work himself, his methods were tested by the clinical results and not by laboratory experiments. Matters on the scientific side had not really advanced very far and Lister still maintained the wide dictum that the septic diseases of wounds were due to putrefaction of the discharges from the wounds. Bacteriology had scarcely begun in this country; indeed, as active workers on bacteria at the time, I can only recall the names of Klein and Burdon-Sanderson.

When I began bacteriological work in a little passage behind the operating theatre in the old Edinburgh Infirmary in 1876 there was no staining of bacteria, no oil-immersion lenses, no solid cultivating media, no proper incubators; in fact, everything was in its infancy, and I had to carry on my observations by the aid of fluid cultivating media, though it was not long before I was able to go over all the work again with a proper microscope and other appliances. I used various media, at first milk, which was soon discarded, and I finally did most of the work with a vegetable (cucumber) infusion or meat infusion. After various experiments I ultimately employed flasks with cotton-wool caps, the flasks being boiled and the cotton-wool steeped in ether and carbolic acid and kept hot in order to drive off the carbolic acid. Later I used Lister's hot chambers for dry heat. I got the material for

inoculation by using capillary (vaccination) tubes; one was picked up by a pair of forceps, sterilised in the flame of a spirit lamp, one end quickly introduced into the opening of the drainage tube, and then withdrawn and dropped into the flask containing the cultivating media.

I need not go into details here, but I may say that I soon learnt to do this quickly, and could reckon on the accuracy of the result. If no growth occurred the fluid remained clear; on the contrary, if organisms had been introduced the fluid quickly became turbid. The examination of the discharge was not confined to the drainage tube, but material was taken from various parts of the skin under the dressing. After some time the methods of staining bacteria were introduced, and I went over the whole work again by the staining method, by which I got further information, especially as to the numbers of bacteria present, but the essential results were the same.

Speaking shortly, the result was that, with the dressings and methods in vogue at that time (carbolic gauze, carbolic lotions, and carbolic spray), the fluid taken from the interior of the drainage tube within the first twenty-four hours after the operation did not or only very rarely set up growth in the flasks, nor were organisms to be found in the stained specimens. The subsequent result depended on the method employed in dressing the wound, on the kind of antiseptics used, and on the length of time that elapsed between the dressings. Unless *special care* was taken, fluids inoculated from the tube at the end of the second day might become turbid from the growth of micrococci in the fluid, and some micrococci might be found in

stained specimens; but on the other hand, if special care were taken, several days might elapse before any micrococci were found in the wound, if they ever reached it at all. Unless the wound had become septic bacilli were not found.

If the specimens were taken from the discharge on the skin at the edge of the dressing (which was always a much larger one than is used to-day), micrococci (and sometimes bacilli also) could always be found after the first twenty-four hours, in decreasing numbers as the specimens were taken nearer and nearer the tube. (The numbers were, of course, taken from stained cover-glass specimens.) The whole facts suggested that the organisms grew in from the edge of the dressing, which after some time would lose all its antiseptic contents and would then not even inhibit the growth of the organisms.

The *special care* to which I refer above is to disinfect the skin around the wound at every dressing and to change the dressing daily for some days. What was done at that time in cases where a drainage tube was used (of course, a wound where no tube is used ought always to heal by first intention) was to have at hand a basin containing 1–40 carbolic acid with a largish swab floating in it. As the dressing was raised and the drainage tube was becoming exposed, the wet swab was thrown over the end of the drainage tube so as to cover it and the line of incision. We then took swabs dipped in 1–20 carbolic lotion and washed the skin over the whole area which had been covered by the dressing, first the swab preventing the antiseptic from getting into the wound; when this had been done we removed the swab over the tube and applied a new

dressing.[1] In this way the micro-organisms which had been spreading inwards under the dressing were, for the most part, destroyed, and those which survived had to begin their attempts again. Whenever they were getting in sight of the Promised Land they were ruthlessly swept away by a bath of carbolic lotion. A true labour of Sisyphus!

The most remarkable fact to me, trained as I had been to look on micro-organisms as the enemies of mankind and the cause of many of the ills to which man is heir, was that these micrococci could be found in wounds which were to all appearance following what we had become accustomed to call an aseptic course, and that, as far as one could judge, they were causing no special trouble. Could it be that the whole theory on which we were working was wrong and that our critics were right when they laughed at the idea that these minute bodies could be the cause of all these septic diseases? Could it be that they were only accidentally present in the wound or were they some special kind of organism distinct from those which cause septic trouble?

Anyway, the fact was that if they got into the drainage tube they certainly did not cause suppuration or any special trouble. Nevertheless, although in the main this was apparently true, I must say that I did not think that the wounds looked quite so well if these cocci were present in any considerable numbers in the wound as if they were absent altogether. If the

[1] At the present time I follow very much the same procedure, except that the lotion in which the swab is placed is 1–2,000 solution of corrosive sublimate instead of 1–40 carbolic lotion, and that after washing the skin thoroughly with the 1–20 carbolic lotion I wash away the carbolic acid by 1–2,000 sublimate solution.

dressing was left on for several days, say for a week, these organisms were certain to be present in the wound and the dressing often had a strong sweaty odour, and the wound often seemed not to be healing so fast or so vigorously as one would have expected; certainly wounds which were dressed every day or every other day seemed to look better and get on more quickly than those which were left a week.

Were these micrococci really as innocent as they seemed to be? I injected the cultivations in considerable quantity into rabbits and guinea-pigs, but they were apparently none the worse for it. To test it still more completely I injected some of them into my own arm and patiently awaited the result. On the first occasion I injected one minim of the cultivation, which must have contained myriads of these organisms. Next day my arm was painful at the seat of injection and a little swollen. A day later I went, from a feeling of duty rather than of pleasure, to look on at the Hospital sports, but I must confess I should have liked to have had my arm in a sling. But that was all: the whole thing passed off quickly without any local abscess or fatal result. I don't remember the details of the second injection, but I think that I did not put in such a large dose and that no special symptoms resulted. I was very young at the time, and I would not advise a repetition of the experiment, for the more virulent *staphylococcus aureus* or *albus* might be present also, a fact which I did not know at the time when I made this experiment.

A very interesting point is that after some days the cultivations in the flasks have a distinctly sweaty odour, showing that this odour is not the smell of pure

sweat, which is odourless, but is a product of the growth of these micrococci on the skin or in the flask. Again, unless the wound was septic we did not find any other organisms. Now at that time looking up the literature I discovered that micrococci had been found in the lymphatic vessels at the spreading edge of erysipelas, in the peritoneal fluids in puerperal peritonitis, and on the valves of the heart in the malignant form of endocarditis, but whether they were causal agents or only accidentally present had not been determined. Further, various cocci producing pigment were known by that time.

From these facts, and from the wide distribution of these organisms over the world, I think that the proper conclusion is that they form a definite species, that they are normal inhabitants of the skin, that their function is to act as scavengers to keep the skin clean, and that they are non-pathogenic. I presume that the pain and swelling that I had in my arm were due to the somewhat acrid products of the growth of these micrococci in the flasks, and were purely the effect of a chemical irritant and passed off when this had been carried away by the lymphatic vessels. The same is probably the reason why the presence of these organisms in quantity seems to delay healing.[1]

[1] Professor Welch of Johns Hopkins University, Baltimore, has also come across this organism, and has published a paper on the micro-organisms in wounds in *The American Journal of the Medical Sciences*, vol. cii, p. 439, November 1891. He gives it the name *Staphylococcus epidermidis albus*, and has also noticed its frequent presence in wounds which are following an aseptic course, and he regards it as an attenuated form of the *Staphylococcus pyogenes albus*, but he does not mention the sweaty odour of the cultivations of this organism, very probably because he used cultivations only on solid nutrient media instead of liquid media as I did, and that seems to me a very important point in considering the classification of this organism. Further,

In my original paper at the Pathological Society I made a wrong deduction. I pointed out that in the aseptic wounds if organisms were present I found only cocci, whereas if the wounds were septic, bacilli (or, as I then termed them, bacteria) were also found. Hence I drew the conclusion that cocci were innocent and bacilli noxious. This was quite wrong ; as a matter of fact, the cocci are the real factors in causing the main septic diseases, *e.g.* erysipelas, septicæmia and pyæmia, while the various forms of gangrene and tetanus are due to bacilli, but it is very doubtful whether the bacteria which cause putrefaction can really spread into the living tissues ; most probably they are only injurious by the products of their growth. They are, perhaps, also scavengers like the cocci on the skin.

The frequent presence of these skin organisms in the wounds as compared with any other is no doubt due to the fact that they are all over the skin and are at hand and ready to spread in under the dressing at once as soon as the latter has lost its store of antiseptic, whereas the septic cocci are only accidentally present. But the method of spread of these skin cocci into the wound would assuredly be followed by the septic cocci if they happened to be in the neighbourhood, so that the fact that the organisms which usually get in are not pathogenic must not lead to any lack of vigilance in disinfecting the skin not only before but also after operation, and at each subsequent dressing, and this would, of course, also hold good as regards bacilli.

the fact that it seems to be present in the skin all the world over is also strongly in favour of the view that it is an independent specific organism and not merely a degenerated race of very virulent pyogenic staphylococci. I believe that they are Nature's arrangement for the cleansing of the skin.

But it is another part of the explanation why union by first intention may occur in some cases without any or with only very imperfect antiseptic precautions.

The question of the mode of spread of these micrococci into wounds seems to me to be a very important one and to point to a very marked loophole in the methods of those who abolish chemical antiseptics from their armamentarium, and may explain the stitch abscesses of which they complain so much and which, I can only say, I have seldom been troubled with. The so-called aseptic surgeon, having no antiseptic lotions at hand when he dresses a wound, can only wipe the skin around the wound with dry sterilised cloths. That, of course, will not remove bacteria, and the one great objection to a rigid abstention from antiseptics is that the surgeon cannot rectify a state of matters such as this. For the bacteria which have been spreading towards the wound, not being removed when the dressing is changed, have a shorter distance to travel, and if it is a case where drainage is necessary the chances of sepsis are very great.

It is especially in the case of tuberculous abscesses (*e.g.* psoas or lumbar abscesses) that this is so important. That is one reason why I have always so strongly advocated the washing out of these abscesses and the subsequent stitching-up of the small wound, with or without the injection of the glycerine-iodoform emulsion. That gives the patient the chance that healing may occur without sepsis, and if it should be necessary the operation can be repeated several times, and often with satisfactory results, whereas to open the abscess and drain it *without using antiseptics* is certain to end in sepsis and disaster.

Of late years, I suppose as having written a good deal on tuberculosis, I have been consulted many times as to spinal disease where abscesses have been opened and where sepsis has occurred, and I find that the opinion is very prevalent now that to open a psoas abscess and drain it is, as a *matter of course*, to have a septic sinus. But what a sad state of matters! One of Lister's earliest triumphs—indeed, the first case where he operated antiseptically through unbroken skin—was a case of psoas abscess, and during his subsequent career he had many cases and many successes. It is true that the wounds are usually several months in healing, but the patients pick up health and strength as soon as the abscess is opened provided the cavity remains aseptic. Occasionally a case has become septic—no doubt due to some error on our part, for it is by no means easy to be constantly careful over several months—but such accidents have been very rare. But then we used antiseptic lotions and were very careful at every dressing to disinfect the skin and remove the old epithelium around the opening for the drainage tube, for this epithelium harbours micro-organisms. I look on the result obtained after opening and draining a psoas abscess as the test whether a man is a true aseptic surgeon (*i.e.* a surgeon who can always depend on getting an aseptic result) or not, and I am afraid he cannot come up to the standard if he does not avail himself of the aid of chemical antiseptics.

In the autumn of 1877 Lister migrated to London, and during the winter 1877–78, when I acted as his house surgeon at King's College Hospital, I continued the study of bacteria in relation to wounds and also investigated the question of the presence or absence of

bacteria in the healthy living body. The latter is referred to in my paper at the Pathological Society in 1878 and in my book on Antiseptic Surgery.

By this time a number of pathologists in various countries, stimulated no doubt by Lister's writings, were working at the infant science of bacteriology. Rindfleisch in 1866 referred to bacteria in the organs of patients dying of pyæmia, but he speaks of them as 'vibriones'; Klebs also speaks of bacteria, and von Recklinghausen and others of micrococci in the lymphatic vessels of the skin in cases of erysipelas. A good many other pathologists might be mentioned, but the definite relation of bacteria to infective diseases of wounds was first absolutely demonstrated in the classical work of Koch published in 1877. This work was so important that I translated it for the New Sydenham Society and it was published in 1880.[1]

In his book Koch begins by calling attention to two very important points in technique which have done much to accelerate the progress of bacteriological science. The first is the use of Zeiss's oil-immersion lenses and Abbe's condenser, both being made of special glass. The second is the use of aniline dyes (first introduced by Weigert and elaborated by Ehrlich), such as methyl violet, fuchsine, etc., to stain the bacteria and thus render them visible, and by methods of double staining to differentiate them from the tissues. In the third place he threw light

[1] See *Etiology of Traumatic Infective Diseases*, by Robert Koch, New Sydenham Society, 1880.

on the way in which germs, with which Lister had contended so successfully, brought about disease, and he showed that there were several different kinds of infective bacteria apparently as distinct from each other as the *Bacillus lactis* is different from the organisms which produce other kinds of fermentation of milk. Among the diseases which he produced by the injection of varying amounts of putrefying meat infusion subcutaneously into mice and rabbits were—a septicæmia in mice due to minute bacilli which are found in the blood and especially in the leucocytes in large numbers; a progressive gangrene in mice due to streptococci; pyæmia in rabbits due to micrococci; septicæmia in rabbits due to oval organisms, to which he gave no name, and erysipelas in rabbits due to bacilli.

The importance of this research by Koch as regards Lister's work lay in the fact that he demonstrated that there were various organisms in putrefying blood which were capable when injected into mice and rabbits of setting up each its own definite disease, some of them resembling very closely the different septic diseases which occur in man. The probability, therefore, was that the different septic diseases in man, *e.g.* erysipelas, pyæmia, septicæmia, suppuration, and various forms of gangrene, are due to specific organisms, and that though present in putrefying material they have no necessary relation with the organisms which produce the putrefactive fermentation.

Very shortly after the appearance of this book Koch introduced the methods of cultivating bacteria on solid media, which at one stroke simplified

the budding science of bacteriology to a very great extent.

The real bacteriological value of the spray, and the question whether he should go on with it or not, was a point which exercised Lister very much indeed during the early 'eighties. At the International Medical Congress held in London in 1881 Professor Bruns of Tübingen read a paper with the attractive title 'Fort mit dem Spray' ('Away with the Spray'), which seemed to suggest its abolition. But it turned out that what Bruns advocated was to substitute irrigation with carbolic lotion for the spray, a method which Lister had long before given up, and in the place of which he had introduced the spray as doing the same thing as regards exclusion of living bacteria, with less irritation and less risk of poisoning. It is of interest to quote Lister's own words in reply to Bruns in 1881. Referring to Bruns' suggestion, he said that he would decidedly give his vote in favour of the spray as compared with irrigation, and he went on as follows :

'At the same time it must be distinctly borne in mind that the spray is, beyond all question, *the least important* of our antiseptic means, and that the circumstance that a surgeon does not happen to have a spray producer at hand is no excuse whatever for his abandoning the attempt to obtain aseptic results. But if the apparatus for the spray is at my disposal, I, for my part, do not as yet dare to abandon it. By the careful use of our present means, the spray included,

we have arrived, I think I may venture to say, at absolute security of attaining the great object in view, provided that we have the two essential conditions complied with : an unbroken skin to start with and the seat of operation sufficiently distant from any source of putrefaction to admit of adequate overlapping of the surrounding integument by the requisite dressing. I leave it to those who have done me the honour to visit my wards to judge whether I am guilty of exaggeration in making this strong statement. Such being the case, I should not feel justified, except on perfectly established grounds, in omitting any part of the machinery by which results so important to our fellow creatures have been arrived at.

'Nevertheless I am aware that concomitantly with the perfecting of the spray there has been an improvement in other parts of our antiseptic arrangements, and I am not prepared to say that our increased uniformity of good results may not be due to the latter rather than to the former. And it may be, for aught I know, that when the International Medical Congress next meets I shall be able to speak of results of a still higher order obtained without using the spray at all. For, if further investigation should confirm the conclusion to which our recent facts seem to point, and it should be proved that all idea of atmospheric contamination of our wounds during operations may be thrown to the winds, then no one will say with more joy than myself "Fort mit dem Spray."'[1]

It will thus be seen from this paper, which I think is one of the most interesting of all Lister's communications, showing as it does precise observation,

[1] Collected Papers, vol. ii, p. 279.

acute reasoning, and a very open and unprejudiced mind, that the spray was only a temporary phase in the development of Lister's technique, and that from quite an early period Lister was contemplating its abolition.

Koch's experiments on antiseptics, which were published shortly afterwards, showed clearly that the spray could not possibly kill resistant spores,[1] but the answer as regards naked organisms was not so definite. As the result of investigations which were going on at this time it was also becoming evident that the spores that are present in the air are for the most part the spores of fungi or of such organisms as *Bacillus subtilis*, which are not pathogenic to man.

As to the spore-bearing organisms pathogenic to man, it is quite uncommon to find them in the air ; the only two likely to be found in the dust in the air are anthrax and tubercle. Anthrax spores are not uncommonly brought to this country from the tropics, and, as regards the air, they are chiefly of importance in factories where combing and cleaning of wool goes on ; they are not at all likely to be present in the air in operating theatres or hospital wards. Tubercle bacilli, on the other hand, are not uncommon in the dust in rooms where patients suffering from phthisis have been living, and would, no doubt, be present in the air if the dust were disturbed, but it is very improbable that they would be present in an operating theatre. But even if they were, I never heard of any occurrence

[1] *Mittheilungen aus-dem Kaiserlichen Gesundheitsamt*, vol. i, 1881.

which indicated that tubercle bacilli floating in the air had infected an external wound, although under suitable conditions they may set up tubercle of the lungs (inhalation tubercle).

But although Koch showed that spores were very resistant to chemical antiseptics, the case is different with rapidly growing bacteria and micro-organisms which do not produce resistant spores. After all, the chief septic diseases of wounds are due to micrococci, which have not the resisting powers of the spores of bacilli, and it might quite well be that these, the essential enemies but the more delicate ones, might succumb to the carbolic acid in the spray. A great number of the micrococci which are met with in the air are saprophytes, and are unable to grow in wounds, but the septic froms (*Staphylococcus pyogenes albus* and *aureus* especially) are not very uncommon in the dust of the air, so that one cannot say that there is no danger to be feared form air infection. Indeed, were it not for other factors, notably the question of dosage, to which I shall refer presently, and the power of the defensive arrangements of the human body to overcome small numbers of these pyogenic cocci, the danger of infection from the air by these organisms would be very considerable and by no means negligible.[1]

[1] As regards the value of the spray in destroying non-spore-bearing organisms, I performed a large number of experiments (not for publication but for our own information), but I did not find it at all easy to come to definite conclusions. Both fluid and solid cultivating media were used, and in some respects I think that the results obtained with fluid media were the more dependable, and for this reason: The dust in falling through the spray would necessarily become bedewed with the spray, and if dust so bedewed fell on a solid cultivating medium there might be enough carbolic acid adhering to the particles of dust to prevent the germination of the micro-organisms (for a small quantity of carbolic acid is sufficient to inhibit growth), while if the dust had fallen into a fluid medium the

But as a matter of fact the greatest danger of infection of wounds from the air is not from floating dust but from the individuals concerned in the operation itself or the onlookers. Speaking, coughing, sneezing, etc., mean the expulsion of particles of fluid from the mouth laden with micro-organisms, many of them highly infective, and unless special care be taken the wound may be quite readily infected, even though at some considerable distance from the culprit.[2]

carbolic acid would have been quickly diffused throughout the fluid, and would not have affected growth at all. My plan was to arrange groups of flasks at various distances from a steam-spray producer, start the spray, quickly take the cotton-wool plugs out of each group of flasks at different times and, after the lapse of a certain time, reinsert them, and place the flasks in an incubator. I have lost my notebook of these and many other experiments done for our own information, and cannot give exact statements, but in the experiments done in the above manner a considerable number of the flasks remained uncontaminated, and a still larger number of the plates with solid media. There are three possible explanations of these results, viz.:

1. The spray, rushing across the mouths of the flasks or of the dishes in which the glass plates with the solid cultivating media were placed, swept the dust away with it, or moistened the dust so that it stuck to the rim of the flasks and did not fall into the cultivating medium. I think that this is the explanation of the majority of the negative results.

2. The bacteria were bedewed with so much carbolic acid that their growth in the cultivating media was inhibited. As I have already said, this might quite well be the case with solid media, but not with fluids.

3. The organisms were killed. I do not think that this is so unlikely as has been supposed. After all, some of the organisms must become bedewed with the carbolic acid while swirling about in the air, and, even after settling the bombardment with carbolic acid would continue and might quite well prove effective in destroying a considerable number.

[2] In order to test this I started a steam spray as before, and then, when the assistant was ready to uncover the flasks, I took a small hand spray-producer containing some very diluted putrefying fluid, or cultivations of definite organisms, and sprayed these fluids at a higher level over the carbolic spray, but in the same direction. The spray made by the hand machine was not so fine as that with the steam spray, so that the particles from the former fell through the carbolic spray, and a large proportion of the flasks became infected. Hence, if the surgeon speaks over the wound the spray would not rectify matters, and it is not always easy to operate in silence before students and visitors.

It was, therefore, quite clear in the early 'eighties that something should be done about the infection from the mouth to which I have referred above, and that we should be careful about speaking during an operation. As regards this point I myself was very careful for some considerable time before masks were introduced. If I wished to speak to the onlookers I took a large swab or towel out of the 1–2000 sublimate lotion and threw it over the wound, and then turned my head away from the wound while I said what I wanted to say. But, apart from demonstrating things, one is always wanting something handed or something done during an operation, and for this purpose I used the expression 'imphim,' which does not involve opening the mouth at all; indeed, the sound cannot be produced with the mouth open. Very soon you can educate your nurses and dressers to do what you wish by different intonations and actions in producing the word. This used to be of great interest and give much pleasure to distinguished visitors, and I think that it was an extremely useful precaution.

There was, however, another reason apart from the destruction of germs in the air which made Lister hesitate to abolish the spray early in the 'eighties, and that was, that surgeons had not yet become very expert with their antiseptic methods; and, indeed, even then Lister's principles and views as regards bacteria were not generally accepted, so that many loopholes were left for the entrance of sepsis, and it was quite likely that the spray, by keeping the hands

and instruments and towels and the wound itself constantly bedewed by a thin film of carbolic acid, might just save the situation, either by killing the bacteria outright or by inhibiting their growth till the tissues were able to deal with them. Though it was clear that we must not assume that the spray killed all the organisms in the air, nevertheless it had this great advantage, that it kept everything around the wound constantly moist with carbolic lotion and possibly, therefore, non-spore-bearing bacteria falling on these towels, etc., might very soon be killed. Hence I believe that it was quite a useful weapon in the early days; but with investigations with solid media and the rapid development of bacteriology and improvement in technique, such proofs were offered to Lister, especially in the more careful examination of the air and in the facts about the dosage of bacteria to which I shall refer immediately, that in 1887 he decided to give up the spray without substituting any antiseptic irrigation for it, and he never had any reason to regret that step. Indeed, in the days of the spray there was considerable hesitation in opening the abdomen under the spray and bedewing the intestines with carbolic acid, thus irritating the peritoneum and, it might be, seriously lowering the temperature of the abdominal contents, and thus increasing shock, but on its abolition abdominal surgery took a great step forward.

The disinfection of the skin was also a very important procedure, but not one easily tested bacteriologically. Lister from first to last washed the skin at

and for a considerable distance around the area of operation with 1–20 carbolic lotion, and in the case of adults a cloth wet with this lotion was left over the area for a considerable time, even all night if the operation was to be performed in the morning. It is difficult to test the result, for if the skin has been well soaked with carbolic acid, even if it is subsequently washed with sterilised water, one cannot be sure that any scurf one scrapes off does not carry enough carbolic acid with it to prevent the germination of any bacteria which may be there. In some cases, in parts where the skin is thin and not hairy—*e.g.* the anterior surface of the forearm, and the front of the abdomen in children, etc.—if the part is merely well scrubbed with soap and water, without any application of an antiseptic, and then rinsed in sterilised water, one may frequently fail to find organisms in the scrapings of the skin, so that it is not entirely a question of inhibition ; there may be no bacteria there. But in parts of the body where there is hair and large sebaceous glands, simple washing does not remove the bacteria.

I have not tested skin painted with iodine ; it seems to me difficult to get rid of the iodine and so exclude the question of inhibition. Experiments to test the disinfection of the skin have been done very thoroughly by taking pieces of the whole thickness of the skin at the beginning of an operation, after disinfection, and placing them in or on nutrient media, and some say that in most cases they find cocci ; those are very likely the same organisms of which I made the acquaintance in examining the discharges from wounds (*Staphylococcus epidermidis*). Lister always had

great faith in carbolic lotion as a means of disinfection of the skin, and he judged from the clinical result. I have never seen any reason to find fault with it except in the case of children or emaciated adults, especially those suffering from hectic fever ; in both these cases the carbolic acid may be absorbed and produce symptoms of carbolic-acid poisoning. Lister pointed out that carbolic acid combined with oil very readily and looked on this as a very important point, for it enabled the acid to get into the sebaceous glands and attack any organisms which may be there. He used to show his class how if one puts some other lotion or water itself on the skin it runs off like water off a duck's back, while carbolic lotion does not do so, but wets the whole area to which it is applied, and so has time to act. Hence he held that carbolic lotion comes into better contact with the organisms than most other antiseptics.

I have already referred to Lister's strong views that the tissues or fluids (or both) of the living body must in some way or other oppose the attack of bacteria ; indeed, bacteria are so numerous and ubiquitous that life could not exist if there were not some powerful mechanism for preventing their entrance into and their development in the living body. The mouth and alimentary canal are teeming with bacteria, but if the individual is in good health they seem unable to get through the epithelial layers. Lister also extended this protective property to blood clot and serum outside the animal body.

As this matter is very important I had better quote

his paper,[1] published in 1881. He is trying to explain why operations for ovariotomy succeed so often without the use of antiseptics.

'At the Cambridge meeting of the British Medical Association last year I brought forward facts which showed that the serum of blood is not at all so favourable a soil for the growth of micro-organisms as I had previously imagined. If we take a glass of uncontaminated milk or urine, so arranged that if left untouched it will remain for any length of time free from organisms, and add to it a drop of ordinary water, we are sure to find, in a few days, evidence of bacteric development in the liquid. Indeed, in the case of milk, which appears to afford pabulum for almost all varieties of micro-organisms, I have shown that if a dozen glasses of the liquid in a state of purity, in vessels suitably arranged to prevent contamination from without, receive each $\frac{1}{100}$ of a minim of tap water, most of the glasses will develop bacteria, though of different species in the different vessels, showing how numerous and how various are the micro-organisms really present in water. Even the comparatively crude liquid which we call "Pasteur's solution," a mere solution of cane-sugar, tartrate of ammonia, and earthy salts in which many kinds of bacteria refuse to grow at all, will be pretty sure to produce such organisms in a few days if a drop of tap water is added to it. Now from these analogies, and knowing as we do to our cost that blood serum is but too liable to putrefactive fermentation, I had assumed that ordinary water contained putrefactive bacteria in a form that would develop in serum.

[1] Collected Papers, vol. ii, p. 278.

'But when in the course of an experiment to be again referred to, I drew blood, with antiseptic precautions, from the jugular vein of an ox into a series of purified bottles, about half an ounce into each, and, having allowed the blood to coagulate and the clot to shrink, introduced various quantities of tap water to mingle with the expressed serum in the several vessels, I found to my surprise that not only an entire minim but two, four, and even eight minims failed to induce putrefaction, although the bottles were kept in a warm box at the temperature of the body. I have since confirmed this experiment in the ox and have also extended it to the blood of other animals—the donkey and the dog—with similar results. I even found that putrid blood in full activity, if largely diluted with water purified by boiling and introduced in small quantity in proportion to the serum, failed to occasion putrefaction or the development of any organisms that I could discover by ordinary microscopic examination. Yet the same quantities of the same dilutions quickly give rise to putrefaction in blood of the same animal altered by mixing it with an equal part of purified water, showing that they really possessed septic energy though unable to exert it upon normal serum. Not that the blood of these animals was in its natural state incapable of putrefaction, for inoculation with a very small quantity of undiluted putrid blood soon rendered it highly offensive.'

I was very much interested in this discovery of Lister's and often pondered over it. Up till then I had thought that bacteria developed so quickly that it would not matter, if they were placed on a suitable soil, whether 1 or 100 were planted at one time, or

if it did matter it would only be a question of time, the result of inoculation with 100 being apparent some time before the result with 1. But here the point is that when too few in number the bacteria never grow at all. I did not happen to hear Lister's paper at the Congress, as I had been detailed to attach myself to Koch and take him about, so that it was some considerable time before I was able to read it. The question then occurred to me whether this would hold good in infective diseases, whether in fact the occurrence of an infective disease, say tetanus, depended in any way on the numbers of bacteria introduced in the first instance or not. I did some experiments on the subject in 1884, but it was not till 1886 that I was able to study this matter fully.[1]

At the end of 1885 I made some preliminary experiments on this matter with *Bacillus proteus*, and, finding that a study of this question was likely to lead to very interesting results, I accepted an invitation from Koch to go over to Berlin in the spring of 1886 and work out the question in his laboratory; and I was very much indebted to him and his staff for their very valuable and cordial assistance. I need not go into detail as regards these experiments, but it was clearly shown that there is such a thing as dosage of infective bacteria, and that both the occurrence of infection and the severity of the disease so produced depended partly on the number of bacteria introduced in the first instance and partly on the resisting power of the species of animal used for the test. Take anthrax, for example. Guinea-pigs are very

[1] Report on a study of certain conditions of infection, *British Medical Journal*, July 31, 1886; and *Suppuration and Septic Disease*, Young J. Pentland, 1889.

susceptible to anthrax. By continued dilution of blood from an animal which died of anthrax I was able to inject quantities of fluid which ought to have had only one bacillus to one c.cm. In the case of two animals into each of which one c.cm. was injected, one remained unaffected and the other died of anthrax; in the first case there were probably no bacilli in the fluid injected.

The results were the same with mouse septicæmia in mice—one bacillus killed a mouse—and the same with chicken cholera in chickens. But the fatal dose of chicken cholera in guinea-pigs lay between 150,000 and 320,000 bacilli. As regards tetanus in rabbits, at least 3,500 bacilli must be introduced to produce a fatal result; smaller numbers may either have no effect, or they may cause a local tetanus in the limb into which the organisms are introduced, which may pass off and the animal recover. Of *Staphylococcus pyogenes aureus* in rabbits, about 1,125,000,000 were required to kill; one-tenth part of that produced an abscess from which the animals recovered.[1]

[1] I need not mention more of these experiments, but I drew several conclusions from them as follows:

1. The pathogenic dose of a virus varies inversely with the predisposition of the animal to the disease in question.
2. In animals not very susceptible to a disease, the severity of the affection varies directly within certain limits with the amount of virus introduced.
3. Up to a certain point the length of the incubation period varies inversely with the amount of virus introduced.
4. In some instances a small dose protects the animal from the fatal effect of a larger dose.
5. The effect of the virus depends in some cases on the tissues into which the dose is injected: for instance, it is generally greater when the injection is made into muscle.

It looks as if in an animal not very susceptible to a disease a small dose gives the body time to mobilise its forces and either destroy the intruder or damage him so much that he cannot do any great harm and ultimately dies out; whereas if the defensive arrangements are weak no such thing will happen, even with a small dose.

It will be quite clear that the fact that the initial dose of bacteria influences the result is a very important one and plays a very important part in the matter of infection. And there is another point to which I should call attention, viz. that the dose must be concentrated, and that where one says that 1,000 bacteria were required to set up a disease that is the result if they are all acting in one area, and I doubt if the same result would necessarily follow if 100 bacilli were put in ten different and widely separated parts of the body. I must, however, admit that I did not go into that matter very fully.

I need not, however, take up space in discussing these results. If we ponder over them carefully, it will be seen that they are of very wide importance and explanatory of a great many things which puzzled us very much in the early days, and which had to do with the delay in the acceptance of Lister's views and methods. In arguing in favour of the meticulous care that Lister took to avoid sepsis, and of the theoretical basis of the treatment on which Lister's methods were founded, one used often to be met with the statement that healing without suppuration or septic disease was no new thing, and that therefore Lister's views could not be accurate. Some asserted that any increased success which Lister obtained was essentially the result of the increased simple cleanliness (apart from disinfection) that Lister's method entailed, and as time went on those surgeons who were particularly careful as to cleanliness were able to bring forward very striking results in favour of this view. And there is no doubt that there is much truth at the bottom of this contention. For even simple cleanliness (and

by that I mean simply washing with soap and water) means a very great advance over the state of matters which prevailed before the Listerian period. In the pre-Listerian days I have no doubt that many of the bad results, such as hospital gangrene, erysipelas, etc., were due, to a very great extent, to want of cleanliness. In those days the surgeon kept an old frock-coat hanging up outside the door of the operating theatre, which he put on on his arrival at the hospital; it was covered with dried blood and dust and dirt, and the bloodier it was the prouder was the surgeon. He turned up the sleeves and the coat-collar, and was ready to operate. The instruments were laid out on a table; no doubt they were washed before they were put away, but there was no special preparation of them before being used again. I have seen a surgeon, very proud of a pocket bistoury which had belonged to a great surgeon before him, put his hand in his waistcoat pocket, take out the knife, open it, and perform the operation. At the end, it would be wiped with a damp and then with a dry towel, shut up, and replaced in his pocket ready for the next operation. And in the ward those attending to the wounds went from one case to another with the same instruments, and unwashed hands, spreading infection as they went. Those who held that Lister's work was merely the result of simple cleanliness were particularly careful in all these matters and consequently had greatly improved results. As the result of their cleanliness they washed away infective bacteria from the seat of the operation, leaving mainly, and often no doubt only, the *Micrococcus epidermidis*, of which I have already spoken; their own hands had been cleaned, but not so

successfully as the skin elsewhere, because of the nails and folds and wrinkles about the hands and of the fact that no part of the body is more frequently or intimately associated with septic material than the fingers. By this washing the masses of the bacteria were broken up, and the dose of bacteria was much smaller than it otherwise would have been. Further, the bacteria naturally floating in the air are mostly non-pathogenic. And lastly, we have to reckon with the resisting power of the tissues, which is a most important point and varies in different situations and in different people. The best successes with simple cleanliness are obtained in operations in the peritoneal cavity, which, instead of being the most dangerous part of the body, is really just the opposite.

Hence it may quite well be that a considerable number of cases might do well although not clean bacteriologically, but the very best results of simple cleanliness are in no way comparable to those obtained by strict antiseptic treatment. The percentage of success is not nearly so good and the whole thing is a lottery, there being no certainty whatever whether an individual case will do well or not. Seeing that the antiseptic treatment of wounds gives more certain and more favourable results, I do not think that it is justifiable at the present day to subject a patient to a chance when there is no reason whatever why the surer method should not be taken. I doubt very much if any surgeon at the present time would do so.

I think that as an example of Lister's open-mindedness and readiness to look at all these problems from an unbiassed point of view, his remarks on the work of Dr. Thomas Keith and Sir Spencer Wells are very

striking. Dr. Keith was a great personal friend of Lister's and had achieved much success in ovariotomy by scrupulous cleanliness and care of the patient during and after the operation. The following are the remarks that I wish to quote[1]:

'Mr. Spencer Wells and Dr. Thomas Keith achieved results which astonished the world before strict antiseptic treatment was thought of: and when several years ago Dr. Keith expressed to me an intention of performing ovariotomy antiseptically I strongly dissuaded him from his purpose; I knew his already brilliant success; I felt that our spray apparatus[2] was as yet inadequate for the production of a cloud sufficiently large to cover the whole field of operation and sufficiently fine to avoid needless irritation; and I was also aware that such operations are often both very protracted and very anxious, while in proportion to the duration and the anxiety of an operation is the chance of the neglect of some apparently trivial yet important element in the procedure. And if the antiseptic treatment were attempted in ovariotomy and failed in its immediate object, I felt that it would be not only nugatory but injurious. It seemed to me that in any case of ovariotomy performed without antiseptic measures there was a contest between effusion from the wounded surfaces and absorption of the effused serum by the uninjured peritoneum. If absorption kept pace with effusion there was no time for putrefactive fermentation to take place in the effused liquid; but if absorption lagged behind and effusion predominated, the serum accumulated in the abdominal

[1] Collected Papers, vol. ii, p. 275 (1881).
[2] This was before the steam spray.

cavity and, putrefying, gave rise to septicæmia. Now supposing an antiseptic like carbolic acid to be employed in the operation, the peritoneal surfaces, as well as those of the wound, would be more or less irritated by it, and in proportion to this irritation would effusion be increased and absorbing power enfeebled, till such time as the temporary effect of the carbolic acid had subsided; and if, in spite of the antiseptic means, active septic matter had been introduced, putrefaction and septicæmia would be the natural result.

'At the same time I believed that the day would come when strict antiseptic treatment would prove valuable in ovariotomy. Especially did I anticipate that it would permit early operation for small tumours, instead of the patient being kept waiting in anxiety, as used to be the case, till the tumour should have attained large dimensions, . . .

'But how, it may naturally be asked, can the success of ovariotomy performed without the use of antiseptic means be reconciled with the truth of the antiseptic principles? The answer is, I believe, to be found partly in certain peculiarities of the abdominal cavity and partly in circumstances common to wounds in general. One great peculiarity of operation wounds within the abdomen, as compared with those in ordinary situations, is that already referred to—namely, that the plasma from the cut surface is poured out into a large cavity lined with a serous membrane disposed to absorb it as fast as it is effused. Thus, without drainage or any outlet whatever for discharge being provided, the serum is, under favourable circumstances, prevented from accumulating as it would in

ordinary wounds similarly treated, and opportunity is not afforded for putrefaction.

'Another favourable result of the disposition of the parts is that, even if some accumulation of fluid does take place, the large size of the cavity, naturally adapted for variations in capacity, prevents the occurrence of tension which is so common a cause of disturbance in ordinary wounds. More especially if a large tumour has been removed, the part affected is left in a state of the most perfect flaccidity and relaxation.

'A further peculiarity in favour of abdominal wounds is due to the high vital power of the peritoneum. I recollect making a post-mortem examination in a case of strangulated hernia where death had taken place within forty-eight hours of the operation, and finding it impossible to discover the site of the incision by inspection from within the abdomen, so completely had the peritoneal wound already cicatrised. This high degree of vital energy operates beneficially in a manner which I shall be better able to explain when I have spoken of some circumstances common to all wounds in their relation to septic agencies.'[1]

[1] It is interesting to note some of the results after ovariotomy in the early days. Dr. Thomas Keith, in spite of the advice which Lister gave him and which is referred to above, did take up Lister's technique as far as he could. Before doing so he had reduced his mortality for the last fourteen years to 1 in 7, but it varied a good deal from year to year. Thus, for the whole of the five years preceding his attempt at antisepsis it was 1 in 10½, but in the last of these five years it went down to 1 in 21. After taking up antiseptics he had a mortality of about 4 per cent.

Spencer Wells published, in 1882, the records of the last 1,000 cases which he had done. The general mortality was 23·2 per cent., the highest mortality was 34 per cent and the lowest 11 per cent. He steadily paid more and more attention to cleanliness of the ward, etc., but he did not use

Up to the present I have been chiefly dealing with Lister's operative work, where he had to do with wounds made through unbroken skin and far enough removed from sources of infection to enable sufficient overlapping of the dressings. In such cases, which at the present time form the chief part of the civilian surgeon's work, we can state quite definitely that septic diseases of all kinds had been completely abolished by the use of the technique introduced by Lister. At the end of his address on the treatment of wounds at the International Congress in London in 1881 he said :

'To provide a condition of our operation wounds that shall put them fully on a par with subcutaneous injuries is plainly the ideal of our art. Towards the attainment of this ideal we have already made large progress ; and towards its full achievement, so far as it be possible, I would earnestly invite the best efforts of my hearers.'

And when he retired in 1893 he could claim that this ideal had been reached in the case of operation wounds through unbroken skin, and that this result had been attained by the use of chemical antiseptics and by a careful technique which we shall discuss presently in connection with more modern suggestions.

any real antiseptic precautions till 1878. He was doubtful as to the value of the spray, but thinks that he got much benefit from the other precautions, and was able to give up the clamp and use ligatures. Probably the abolition of the clamp accounts for the improvement in his results.

Baker Brown substituted the use of the cautery for the clamp as the means of preventing hemorrhage, and his mortality at once fell to half; in his first 50 cases by this method it was 8 per cent., and in 1880 Dr. Thomas Keith states that 98 of his last 100 cautery cases had recovered.

Before going further in this direction I must take up another and very important part of the surgeon's work and make some remarks about two other classes of wounds where the principles of treatment must be modified to a considerable degree. These are : (1) where the wound is made accidentally, or at any rate not by the surgeon, and where some time elapses before the surgeon sees the patient and can deal with the case ; and (2) where operations are performed by the surgeon which open up septic cavities or lead to effusion of septic material over the surface of the wound, with possible infection.

As regards wounds inflicted not by the surgeon, but by accident or by various external causes, the gravity of these injuries varies very much, according to the cause of the injury, the structures injured, the sort of infective organisms which get into the wound, and the length of time which elapses before the patient comes under the care of the surgeon.

For example, the cause of the injury may determine the extent and the amount of infection to a very important degree. In a case of compound fracture, if the wound in the skin is due to the broken ends of the bones perforating the skin, the chance of soiling of the wound is not necessarily very great or bad, and, if seen in time, it may be quite possible to enlarge the opening and eradicate the infection. If on the other hand the fracture is caused, say, by a heavy wheel passing over the limb, dirt may be ground into the parts to a great extent, the soft tissues bruised and

devitalised, and consequently the chances of eradicating the infection are not so good. Again, if the injury is due to a shell or gunshot injury the destruction of the tissues is much greater, the septic material is driven much further into the tissues, and the chances of disinfection will be very poor.

As regards the structures injured, the gravity of the case often depends much on this point. For example, perhaps the worst injury from the point of view of sepsis is where a healthy joint is broken into ; next come injuries of bone, then injuries of muscle, and, last and least serious, injuries of cellular tissue.

The nature of the organisms which gain entrance, such as tetanus bacilli, *B. Welchii*, and other gangrene-producing organisms which are especially associated with soiling with earth, is a matter of supreme importance from the point of view of possible disinfection.

Lastly, the time which elapses before the case comes under the care of the surgeon is a very important point as regards the possibility of eradicating infection. A calculation as to the rapidity of growth of bacteria gives some idea as to the importance of this point. For example, if the bacteria divide once in an hour, in four hours there will be sixteen times as many as at first, in six hours sixty-four times as many, etc., while, if division takes place three times in the hour, the numbers will be 2,000 times as many at the end of four hours, 131,000 times as many at the end of six hours, and so on. This is, of course, only a rough calculation, as it may quite well be that some time must elapse before the bacteria can accommodate themselves to the new food, etc.

From the above statements we see that these

injuries are likely to be most serious in war surgery and least serious in civil practice, because the severity of the injuries are much less in the latter than in the former; the chances of excessive soiling (*i.e.* large dosage with infective organisms) are also less, and the patient comes under treatment much sooner. As Lister had no experience of war surgery, I shall not deal with that subject here.[1]

Where, however, too long a time has elapsed since the injury, or where the septic material has been very widely distributed—where, in fact, there is no chance of rooting out the infection by the application of disinfectants to the wounds—the course advised and pursued by Lister was quite a different one, and I do not think that the difference was ever really understood by those who opposed his views and methods.

[1] This question of time is all-important, and, realising the difficulty of disinfecting wounds in war at an early enough period after their infliction, I made suggestions during the recent war, as to attempts to inhibit the growth of the bacteria for such a time as would ordinarily elapse before the wounded could reach a field ambulance or some place where the wounds could be opened up and, if thought desirable, attempts could be made to disinfect them properly (for experiments on this matter see *Journal Royal Naval Medical Service*, April, 1913). Unfortunately, I was not able to deal with this matter myself at the Front, and our intentions were evidently misunderstood, for the material which we suggested, instead of being regarded as a purely temporary application, to be removed when the patient arrived at the dressing-station or field hospital, was, in a considerable number of cases, applied first at a stage when it was far too late for its use. I have found, on several occasions in civilian practice, that the means suggested have proved most useful, and if ever such a catastrophe as a great war arose again it might be worth while to reconsider this matter, unless, indeed, by that time some method of rendering mankind immune to all kinds of septic disease has been discovered.

As a matter of fact, Lister's teaching from the first was that suppuration was nature's attempt to circumscribe infection and prevent general septic disease, and that nothing should be done at this stage which might seriously interfere with the natural processes of protection. He looked on granulations and granulation tissue as nature's protective wall, and constantly pointed out that in the old days the sequence of events was as follows: (1) putrefaction of the serum and blood in the wound, (2) local irritation by this putrid material, leading to granulation, (3) absorption of the chemical products of putrefaction, setting up fever, and possibly general infection. By the end of the fourth day the granulation wall was usually complete, and then, if no general infection had already taken place, the fever and illness began to subside and the patient was left with a suppurating wound which if treated properly would, in a good many cases, heal in due course. But the greatest care must be exercised in the after-treatment, and, indeed, during the whole treatment, of these wounds not to do anything which might interfere with the process of granulation or injure the granulation wall when it has once been formed.

Among the most serious drawbacks to the healing of these wounds is imperfect escape for discharge from the wound and the consequent risk of the retention of decomposing pus in pockets in the tissues. The constant presence of this decomposing material, often under pressure, is apt to injure the granulation tissue and aggravate the inflammatory process beyond what is necessary, even to the extent of causing local gangrene and increasing the risk of general infection. Hence,

in Lister's opinion, the first essential in the treatment of such cases is to establish free drainage by good-sized tubes, and on the slightest sign of fresh extension of inflammation to make fresh openings to drain the new focus.

A most important point in the treatment of these cases is to deal very gently with the inflamed tissue and to avoid injury to the granulation tissue as much as possible. *Never do anything that makes the granulation tissue bleed*, as that may very readily open up a point of entry for the infective material into the body. I know nothing more trying than to watch someone roughly wiping away pus from the surface of the granulations, introducing probes or forceps to show the course of sinuses, inserting fingers to feel the condition of the ends of bones (such fingers having been very rarely sterilised or even covered with a glove), and so on.

And, just as all mechanical injuries causing bleeding from the granulations should be prohibited, so also must injuries from chemical antiseptics be avoided. The time when there is any chance of rooting out the bacteria (which is the main object of a chemical antiseptic) is long past, and the use of irritating antiseptics at this stage can be of no benefit whatever, and can only result in damaging the protective wall and rendering the tissues less able to keep out infective agents. It may in some cases appear advisable to wash away some of the putrid pus, but it must be done without any force or damage from the syringe or tube. In such cases use normal saline solution or such non-irritating antiseptics as boric acid, permanganate of potash, acetate of alumina, etc. In very foul sloughing wounds, peroxide of hydrogen (about 10 vols.) is

sometimes of distinct value, but it should be carefully used.

As regards dressings, it is well, for reasons already stated, to use dressings impregnated with antiseptics, so as to diminish the decomposition of the discharge; and whenever it is possible, fix the part with splints, so as to prevent mechanical injury from movements.

The only other point to which I need refer is Lister's method of dealing with operation wounds where a communication has been established with an infective area, *e.g.* mouth or tongue. Here, of course, he used all the usual antiseptic precautions, so as to avoid increasing the amount of infective material or introducing fresh infective matter. After the operation, provision was made for free drainage, and Lister was very fond of applying a solution of chloride of zinc to the raw surface of the tongue (not to the edges of the wound), which in his view delayed decomposition (he spoke of it as pickling the tissues), and thus enabled granulations to form and become fairly well advanced before putrefaction had taken place. I cannot say that this seemed to me a strong point, and I am afraid that lately I have generally forgotten to apply the preparation.

For some time before Lister gave up his work, surgeons, especially in Germany, had been making efforts to get rid of chemical disinfectants altogether, trusting to the use of aseptic instruments and materials, along with the anti-bacteric power of the body, to obtain aseptic results. At the International Congress

in Berlin in 1890 several cases were shown which had been treated in this way, but when the dressings were removed all the wounds were found to be suppurating freely. However, just as Lister was not dismayed when his first case died of sepsis, so the attempts above referred to were continued, with greater care. This method has, curiously enough, been given the name of 'the aseptic treatment of wounds,' which is a term difficult to understand. Lister called his method of treatment 'the antiseptic treatment of wounds,' and the results which he hoped to obtain 'aseptic results.' The treatment is antiseptic (against sepsis), and the result is asepsis (free from sepsis). Aseptic surgery is, properly, surgery in which the result is freedom from sepsis; I cannot quite place the term 'aseptic surgeon.' Lister's results and those of the so-called 'aseptic' surgeons are obtained by 'antiseptic' methods. In my book on 'Antiseptic Surgery'[1] I used the term 'aseptic surgery' as indicating methods of surgical treatment with which 'aseptic results' in wounds were obtained as opposed to methods by which sepsis was not avoided, and, of course, no one can contend that Lister's methods did not lead to aseptic results. But now the term 'aseptic surgery' is used to mean surgery in which no *chemical* antiseptics are used, but the words do not express this. The use of the term 'aseptic surgery,' which means surgery by which aseptic *results* are obtained, to imply surgery in which no chemical substances are employed creates great confusion. As a matter of fact both methods are 'antiseptic' methods, and the aim in both cases is to

[1] *Antiseptic Surgery: its History, Principles, Methods, and Results*, Smith, Elder & Co., 1881.

secure 'aseptic' results. It would be far better to speak of the most recent attempts to abolish chemical antiseptics as a further stage in the development of antiseptic surgery and a continuation of Lister's principles. Lister was not tied to any particular technique or any particular mode of disinfection. So long as a method produced an aseptic result he was quite satisfied. Whether the present attempts to abolish chemical antiseptics have not gone too far is, however, a fair subject for debate.

As a matter of fact, Lister very early in his work studied the question of employing dressings which did not contain chemical antiseptics, and may therefore be said to be the first of the so-called 'aseptic' surgeons. As this is known to very few, it may be of interest if I quote the paragraph from his Collected Papers, vol. ii, p. 176, in the address on surgery at the meeting of the British Medical Association in 1870. He was speaking of the theory at the base of his work, and he then referred to Tyndall's beautiful experiment which showed how perfectly cotton-wool deprives the air of its dust.

'The other class of facts in this division of the subject to which I am anxious to direct your special attention was also suggested by one of Tyndall's experiments with the condensed luminous beam—that, namely, in which he proved the perfect manner in which cotton-wool filters the air of its suspended particles by blowing against the beam with a pair of bellows having a mass of the cotton tied over the nozzle; the result being that the beam, elsewhere white from illuminated dust, became perfectly black at the part on which the current was directed through

the cotton filter ; hence the idea naturally suggested itself that cotton-wool might be used with advantage as an *antiseptic* dressing.[1]

'Of course it would be useless to apply ordinary cotton without special precautions, for, according to the germ theory, putrefactive particles must exist among the fibres and lie scattered over the wool. But if the cotton were impregnated with some volatile material capable of destroying the vitality of the septic organisms and then placed upon the wound after washing it with a lotion containing the same substance in solution, the result ought to be, supposing the theory true, that, after the volatile antiseptic had become dissipated by diffusion from the dressing and from the wound, the cotton-wool, though destitute of any chemically antiseptic properties, should effectually prevent, by its filtering property, the access of any putrefactive agents and keep the wound sweet, while in itself a perfectly bland and unstimulating application. Accordingly I prepared four samples of cotton-wool by diffusing through each one of the following substances—chlorine gas, sulphurous-acid gas, carbolic-acid vapour, and the vapour of benzene—four materials very dissimilar in chemical properties but having a common hostility to low forms of life. Chlorine, sulphurous acid, and carbolic acid are well known to have such a property ; and knowing that benzene is used by the entomologist for killing insects, and having ascertained by experiment the potency of its vapour for the destruction of pediculi, I thought it probable that it would also answer our purpose.

[1] Note Lister's use of the word 'antiseptic' in reference to sterilised wool not containing an antiseptic.

'I then dressed with these four kinds of prepared cotton-wool various suppurating sores, excoriations and contused wounds, after washing the surface with the corresponding lotion or, in the case of benzene, with the undiluted material. The result in every instance corresponded exactly with theory. After about twenty-four hours' exposure at the temperature of the body the cotton wool was found to have lost the odour of the antiseptic, yet the blood, serum or pus, as the case might be, remained perfectly sweet for an indefinite period, while healing advanced in the satisfactory manner that might be anticipated from the absence of all irritating quality in the dressings. There was, however, one circumstance, highly instructive in itself, which interfered sadly with the utility of this application; namely, *that if the discharge happened to be sufficiently copious to soak through the cotton-wool and appear at its external surface, putrefaction occurred throughout the entire mass of the moistened part down to the wound even within the first twenty-four hours after the dressing, if the fluid were sufficiently copious to penetrate within that period.*[1] It is only *when dry* that cotton-wool can arrest the progress of microscopic organisms, which have ample room to develop among its meshes when filled with a putrescible liquid.'

In this communication, written long before any real attempts to do without chemical antiseptics were made, Lister indicates already one of the chief weak points of such attempts. It was not till after Lister had given up active work that these methods began to come into vogue, and Lister had no further practical

[1] The italics are mine.

experience of them. But I think that, while he quite saw the advantages of some of the procedures, he dreaded to lay aside the weapons which had proved so successful in his practice in favour of other plans which did not seem so thorough and which meant still greater care and attention to minutiæ of technique than were required under the system which he had elaborated. I cannot, however, give any criticism emanating from Lister of the methods which are in favour at present with the view of obtaining aseptic results, but I should like to state my own views as to the present state of affairs. I shall take the chief points seriatim.

In the first place, those who aim at getting rid of chemical antiseptics use heat as the means of disinfection wherever they can, and the Listerians recognise, as a matter of course, that this is the most effectual means of disinfection that we possess, and it is the one which is universally used in laboratory work. In the so-called aseptic method the instruments are boiled, and the dressings, gowns, towels, etc., are all sterilised in the autoclave. This, of course, we accept at once as a marked advance in our antiseptic methods. There are, however, two points in these methods about which we must be careful, so as to avoid leaving loopholes for the possible entrance of septic material, namely: (1) we must see that the heat is adequate and is thoroughly applied to all the things to be disinfected; and (2) we must see that there is no opportunity *after* the disinfection for the instruments,

swabs, etc., to become again infected before or during the operation.

As regards number (1), it seems hardly possible that there can be much chance of error, nor is there where instruments, etc., are boiled. If the boiling is continued for fifteen to twenty minutes we may reckon that everything will be sterile. But as regards sterilising by dry heat there are two points which must be carefully looked into. As regards the autoclave itself, it is, of course, quite satisfactory in large hospitals, large chemists' establishments, etc.; they naturally get the very best apparatus that they can find. But I have frequently seen in smaller establishments that the apparatus used is very unlikely to do the work efficiently, and I certainly would not trust it myself. Apart from the autoclave, another point is that one often finds that the drums are so tightly packed and so crowded in the machine that it is inconceivable that the temperature in the middle of the drum can be brought up to anything like the necessary degree. Koch, in his classical work on disinfection by steam, is very insistent that a stream of steam shall pass rapidly and continuously through the whole of the material to be sterilised.

For my own part, I accept all these points—sterilised gowns, towels, etc., provided I feel sure that the sterilisation has been properly carried out—but another point arises, namely, that they shall not get infected again before or during the operation.

As regards the avoidance of infection of the instruments, etc., during the operation, I have seen the instruments taken out of the pan, in which they had been boiled, at the commencement of the operation,

and spread out on dry sterilised towels on a table alongside the operating table, where they lay exposed to the dust, etc., during the operation. This can hardly be right, because several people are moving about, disturbing the air, speaking, etc. Nor is it any protection to place them in a dish of salt solution.

I cannot think of any other way of preventing infection of the instruments after their removal from the steriliser than by keeping them till required in an antiseptic lotion. I find that a good many surgeons now keep them in methylated spirit, and that may be sufficient; I always use 1–40 carbolic lotion, and rinse the instruments in 1–2000 sublimate immediately before use, in order to get rid of the carbolic acid.

Again, if an instrument gets soiled—say, by falling on the floor during the operation—one often sees the nurse or attendant pick it up with special tongs, lift the lid of the steriliser, dip in the instrument for a few seconds, take it out, and bring it to the surgeon as sterilised! This is not an imaginary occurrence; it happens with great frequency, and it must be carefully guarded against. For those who will not use antiseptics as disinfectants, the only remedy that I can suggest is to sterilise duplicates of the more important instruments in the first instance, so that there is a second instrument to rely upon, while the one that has been accidentally soiled has its twenty minutes in the steriliser again. For my own part, in such a case I wash the instrument in carbolic lotion and let it lie for three or four minutes in undiluted carbolic acid, and then rinse it in weak sublimate lotion before use.

A further question remains, namely, what about cloths, towels, swabs, etc., during the operation? True, the towels are taken out of the box with sterilised tongs and handed to the surgeon on these tongs, and the box is shut again, or is supposed to be shut again, at once (this is sometimes forgotten). But what if it is a long operation—the towels become bloody, dust continues to fall, and the state of matters around the field of operation will soon be very different as regards asepsis from what it was at the beginning of the operation. Even changing the towels frequently seems to be hardly enough. In this case the chemical antiseptic will with advantage come to our assistance. If at the beginning the sterilised towels are soaked in 1–2000 sublimate solution, wrung out not too thoroughly, and placed over the dry sterilised cloths adjusted next the skin, the dust falling on the wet surface will become fixed and not blown about, often in masses, and, it may quite well be, sterilised. With these restrictions I accept these methods quite willingly.[1]

I would only remark on one other point in this connection, namely, the swabs. Marine sponges seem to be entirely abolished from the 'aseptic' surgeon's armamentarium; I presume that this is because they cannot be boiled. But boiling is not the only way in which things can be disinfected. It seems to be assumed that not only are chemical antiseptics

[1] Before the sterilised gowns came in I had for some time been using a mackintosh apron (which had been well sponged with 1-20 carbolic lotion), buttoning on it in front a large piece of towelling (like a breastplate) which had been soaked and wrung out of 1-2000 sublimate solution, so that if my hands happened to touch it they should not be soiled; and even now I usually put on a mackintosh apron first and then the sterilised gown, and then wet the whole of the upper part of the gown in front with 1-20 carbolic lotion or 1-2000 sublimate lotion, for the same reason.

bad things and to be avoided, but that they cannot be trusted as disinfectants. That some of them are not so potent as was at one time thought may be quite true, but that one cannot disinfect things by their aid is not correct. The victory over sepsis could not have been gained by chemical antiseptics if they could not kill bacteria, or if they damaged the tissues to the extent which they are supposed to do.

As regards disinfection by chemical disinfectants, that depends on the material to be disinfected and the time given for the purpose. If one takes a marine sponge, washes out the sand thoroughly, and puts it into a vessel full of 1–20 carbolic-acid lotion, puts on a well-fitting lid, and leaves it undisturbed for, say, seven days, nothing will be left alive in the interstices of the sponge. Carbolic acid will kill naked bacteria just as rapidly as it kills a white blood corpuscle, and in seven days (and, indeed, in most cases in a much shorter time) one may be certain that all spores will be dead, provided that the fluid can get free access to them. Marine sponges treated in this way can be relied on to be sterile, and the carbolic acid is easily washed out by, say, 1–4000 sublimate solution, and they can then be used for wounds without causing any irritation. They are easily cleansed in the above lotion at the time of the operation, and if well squeezed they convey practically no antiseptic to the wound and are really more certainly aseptic than dry gauze swabs. Further, they suck up the blood at once, which is not the case with the swabs, and there is not the mechanical injury to the tissues which results from the rubbing of the wound with swabs. If used in an aseptic case and subsequently well washed they may be again put into carbolic

lotion for the same length of time as before and used afterwards without fear ; but if used for a septic case I always burn them.

As regards the other details of this method, masks have been introduced to cover the mouth and prevent the escape of saliva, and this is, I think, the most useful of all the new details. I consider that the use of a mask by everyone on the floor of the theatre is most essential. Whether the hair of the head is covered or not seems to me to be immaterial, but it is, I think, more comfortable and fits better if it (the arrangement) includes the whole head. The mask is a great improvement on my old 'imphm.'

As regards india-rubber gloves, I took to them also from the first. It certainly is a good precaution. I believe that the outcry against chemical antiseptics arose in the first instance, and perhaps in the main, from the objections of the surgeon to disinfecting his hands with carbolic or other antiseptic lotions. There is no doubt that carbolic lotion dulls the sensation of the fingers and tends to lead to cracks of the skin, which are painful, while the use of sterilised gloves removes this objection, and there can be no reason against cleansing the gloves in an antiseptic lotion like 1–2000 sublimate from time to time during the operation. But, as with other things, care must be taken that the gloves shall not become infected before or during

the operation. In this connection I often notice a possible loophole in the putting on of the gloves. The surgeon washes his hands in a basin, but does not disinfect them ; he only dries them with a sterilised towel. The gloves are sterilised dry and are handed with tongs to the surgeon. He takes one in his hand and proceeds to put it on, and in doing so he handles it, sometimes with a good deal of squeezing and rubbing, with his unsterilised hand. He then takes the second glove, and the same process is gone through ; the gloved hand, with whatever it may have acquired from the other hand, is used to push and pull on the second glove. It is entirely a lottery whether or not gloves manipulated in this way become infected while being put on.

As far as I personally am concerned, I sterilise my hands and put the sterilised gloves in a basin of 1–2000 sublimate solution, fill the glove full of lotion, and with very little trouble the hand slips into it, and it is very easy to squeeze out all superfluous fluid. In this way not only do we avoid infection of the gloves in putting them on, but we also continue the disinfection of the fingers, which is a valuable precaution in case the glove should tear during the operation. In view of the latter accident, I think it is well to disinfect the hands in cases where this is likely to happen, as in operations on bones, for stone in the kidney, etc. . . . In ordinary cases I admit that complete sterilisation of the hands is not necessary if, after the gloves have been put on, the gloved hands are immersed in an antiseptic solution for a few minutes in order to remove any septic material which may have got on them while they were being put on, but I should feel most

uncomfortable all through the operation if I had not first sterilised my hands.

Lastly, in the modern methods the dressings are sterilised in an autoclave, and consist of cotton-wool and plain gauze, neither of them containing any antiseptic. The disadvantage which arises from the absence of an antiseptic in the dressing is well stated by Lister in the passage which I quoted *re* his early attempts to use dressings which contained no antiseptic. In this passage he stated quite correctly the essential objection to this dressing, viz. that if there is enough discharge to soak through the gauze and cotton-wool there is then nothing to prevent the rapid spread of bacteria into the wound.

In actual practice there are two conditions as regards wounds which have to be considered in connection with the dressings, viz. whether drainage is or is not necessary.

If drainage is not required, the whole wound may be closed by stitches and, unless there is bleeding, there will be practically no discharge. Here quite a narrow dressing, fixed down by narrow strips of strapping or by collodion at the edges, will suffice, but no impermeable material, *e.g.* collodion, should be put on outside the dressing. Bacteria can grow only when there is a large percentage of water in the medium, and therefore anything which prevents rapid drying of the lymph and blood along the line of incision and stitch tracks should be avoided. If, however, a scab forms, no bacteria will penetrate

into the wound, and therefore no antiseptic is required in the gauze.

In the case where drainage is required either by tube or by gauze the state of matters is quite different. If there is much serous discharge from the wound it will soon soak through the dressings, even if they are in large mass, and the moment it reaches the outside, whether by passing directly through the dressing or under it, bacteria begin to grow in it, and it is only a question of time before they reach the drainage opening and infect the wound; this may easily occur within twenty-four hours.[1] Hence to give a chance of keeping out bacteria from the wound the dressing would need to be changed frequently. And not only must the dressing be changed early, in order to prevent the bacteria which are spreading inwards over the skin from reaching the wound, but the organisms on the skin must be killed before putting on a fresh dressing, and this can only be done by washing the surface of the skin outside the wound with a strong antiseptic lotion, preferably carbolic acid.

If, however, the gauze contains a suitable chemical antiseptic, such as the double cyanide of mercury and zinc, which will dissolve in serum, and is in sufficient amount to render the serum an unfit soil for the growth of bacteria, there is no need for such haste in changing the dressing and the risk of septic infection is thus appreciably diminished.[2]

[1] See calculation of rate of multiplication of bacteria.

[2] For example, in the case of an extensive operation for cancer of the breast, I would proceed as follows. After the operation has been completed and the bleeding arrested, I take steps (if necessary after considerable undermining of the flaps and the use of stitches of relaxation) to sew up the whole wound with interrupted sutures, and, finally, with a

I must confess that, puzzle my brain as I may, I cannot understand why the double cyanide gauze, with all the advantages that it offers, should not be used instead of plain sterilised wool or gauze. It irritates neither the wound (indeed, it does not get into the wound) nor the skin, and its advantages seem to me to be self-evident. To refuse to avail oneself of powerful weapons like chemical antiseptics because at one time they were abused (not by Lister or his followers) is to throw away needlessly a very great help in attaining an aseptic result, and it is especially difficult to understand the objections when one

continuous silk stitch. Previously to finishing this stitch a hole is made in the posterior part of the lower flap sufficiently large to admit a fair-sized drainage tube, which runs up as far as the clavicle, and the continuous suture is then finished. The result is that in many cases the wound can be closed, the only opening left being that for the drainage tube. The line of incision is now covered with strips of gauze and sterilised wool and fastened down to the skin by collodion as before referred to, the skin about the tube being then well washed with sublimate solution; a small piece of cyanide gauze, wrung out of the sublimate solution, is placed over the tube, and then a biggish mass of cyanide gauze and outside it of salicylic wool (both sterilised in the autoclave) is applied over a considerable area, the opening for the tube being about the centre of this area, and the whole being steadied by a large, many-tailed bandage. The first dressing takes place in 24 hours (the anterior dressing is not disturbed for a week or ten days); the whole mass of dressing over the drainage tube is removed, a swab dipped in 1-2000 sublimate being thrown immediately over the opening of the tube, and then all the skin around the tube, as far as the dressing extends, is washed with 1-20 carbolic lotion and then with 1-2000 sublimate solution, and a new dressing applied, the tube not having been touched. On the second and third days exactly the same procedure is adopted, but on the third day, after the disinfection of the skin, the tube is pulled half-way out and cut flush with the skin and a sterilised safety-pin is passed through the sides to prevent it from slipping in. Two days may now generally be allowed to pass, and then the same procedure is adopted, but on this occasion the tube is pulled completely out and left out. Usually three days may elapse before the next dressing is carried out in the same way, and then an interval of four or five days, and the case is practically over.

remembers, as I have said before, that the victory over sepsis was gained years ago by these very chemical antiseptics which are now so looked down on.

To abolish chemical antiseptics altogether is like tying one hand behind one's back while fighting the enemy with the other. There is no need for such quixotism, and, even if the antiseptic were not absolutely necessary, it is better to exceed in our precautions than to be wanting in them ; it does not do to despise one's enemy. And finally I would suggest that it is a mistake to teach students that chemical antiseptics are not only useless but disadvantageous, for they may be frequently faced with conditions, in all parts of the world, where they cannot get sterilisers, etc., but where, did they know how to use chemical antiseptics, they might quite easily save the situation.

These are the only very serious loopholes that I need refer to in the more modern methods ; everything else is an extension of, and some are marked improvements on, Lister's technique. But it is all 'antiseptic surgery,' and the hoped-for results are 'aseptic wounds,' and I hope that the tendency to regard this so-called 'aseptic' treatment as something apart from the Listerian antiseptic treatment will disappear. The basis of both is the same, namely, to prevent living septic organisms from entering wounds.

I may sum up the matter by pointing out that the essential differences between the two involve only the two points in technique, not in principle, which I have been referring to, namely :

1. Shall one under any circumstances use dressings containing a chemical antiseptic ? and

2. Shall one disinfect the skin around a wound or the opening for drainage with carbolic acid when changing the dressing, or shall it simply be rubbed with a dry sterilised swab ?

These seem very small differences, but in my opinion they are extremely important. I believe fully that the use of dressings not containing a suitable antiseptic, and the avoidance of antiseptic lotions for disinfecting the skin at the change of dressings, will explain the failures in tuberculous abscesses, especially spinal abscesses, and also to a great extent the troubles about stitch abscesses, sterilisation of catgut, attacks of gout or influenza causing wounds to suppurate, etc. ; these last troubles are things with which I am not at all familiar, but I hear of them. The only difficulty with catgut that has come my way is of recent occurrence and arises, I believe, from over-preparation, and certainly not from sepsis. Nor am I familiar with the stitch abscesses about which so much has been said and written. I have, as a matter of fact, been called into consultation over a few cases in which 'stitch abscesses' were said to have occurred, but which, had they happened to me, I should have attributed to some serious error in my technique.

So far I have been speaking mainly of the technique employed with the view of preventing micro-organisms entering and growing in wounds, and have also indicated various points, apart from this technique, which play an important part in the success of the surgeon. Such points are, on the side of the

bacteria, their nature, their degree of virulence and concurrent growth with other bacteria, and their ability to grow in serum or blood ; and on the side of the body, the degree of natural immunity, general and local depression of vitality, cold, injury, action of irritating chemical substances, the seat of inoculation and anatomical arrangement of the part, etc. I went very fully into these matters in my lectures at the Royal College of Surgeons in 1888,[1] and though that is a long time ago, I venture to think that these lectures are still worth perusal by beginners in this subject and really require very little alteration as regards the facts which form the basis of this important subject, although, of course, they require additions, especially as regards the processes of protection and immunity. I need not, therefore, repeat myself here.

Since that time, however, a great deal has been done on the subject of increasing the protective mechanisms of the body, and this is very important for those who wish to do without the aid of chemical antiseptics. I especially refer to the antitoxic sera and to vaccines. This subject is, however, still in such a very fluid condition that it is impossible to make any clear or definite statements on the subject.

[1] *Suppuration and Septic Diseases*, Young J. Pentland, 1889.

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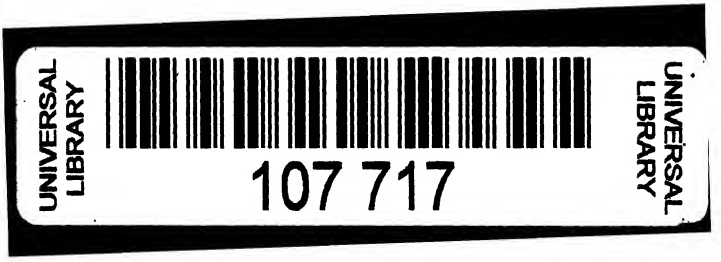
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